THE ANNALS

OF

ELECTRICITY, MAGNETISM,

AND

CHEMISTRY;

AND

GUARDIAN OF EXPERIMENTAL SCIENCE.

CONDUCTED BY

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JULY, 1842.

On the Electric Column and Aerial Electroscope. *By* J. A. De Luc, Esq., F. R. S.

(Continued from page 454, vol. viii).

ARTICLE I.

The principal result of my paper on the Analysis of the Galvanic Pile, has been to show that by this instrument, in which Sig. Volta has so much extended Sig. Galvani's first discovery of some physiological effects produced by two associated metals, we have been really enabled to determine whence proceeded that action upon the animal economy. When these effects were discovered by Sig. Galvani, appearing similar to the shock produced by the Leyden vial, they pointed out some action of the electric fluid; but when this fluid acts thus upon our organs, it is also manifested by electric motions and by sparks; whereas not even the first of these signs appeared in the galvanic experiments. Therefore the action of the electric fluid in these first phenomena might for ever have remained doubtful, had not Sig. Volta, by the invention of his admirable pile, increased that action, so as not only to be attended with electric motions and sparks, but to produce some chemical effects known before to belong to the electric fluid.

But there remained a great question. These different effects had before been produced by the electric fluid only when it was arrived at a great density; while the pile produces the same effects with so small a quantity of the fluid, as to be often hardly sufficient to move the gold leaf electroscope. This has been the object of the experiments contained in my first paper, which have manifested two distinct effects in the pile: 1. A motion of the electric fluid produced by the association of two proper metals, independent of any other effect: 2. A modification of this small quantity of electric fluid, on pervading the pile during the calcination of some of

its metals. A distinction first shown by different dissections of the pile, and afterwards directly proved, by producing an instrument, which retains the electric effects of the pile by the association of two metals, without either its chemical effects, or the shock.

This new instrument was to be distinguished from the galvanic (or voltaic) pile; therefore, in my second paper delivered to the Royal Society on this subject, I named it electric column, as being a spontaneous and permanent electric machine; and it manifested also, by changes in its electroscopes, some variable influence of the electric state of the air, this new effect was to be expressed by an additional name. It is different from the indications of our former atmospheric electroscopes, such as elevated conductors and kites, which inform us only of the comparative states of the stratum of air that they attain, and of the air at the place of observation; without any indication of the changes in the latter, probably connected with some phenomena which we observe, without knowing their cause; and as the electric column seemed to promise a method of discovering these changes, I named it also aerial electroscope.

Such had been the principal object of my second paper presented to the Royal Society; it was only in its nascent state; but as I thought it worthy to be taken up and followed by other experimental philosophers, I would not postpone communicating it to the public till it was more advanced; it has not been published in the *Phil. Trans.*, and as I have since carried it further, I shall here treat it in a different manner, dividing it into three parts; the first will relate to the electric column, considered only as the electric efficiency in the pile, divested of either chemical or physiological effects. The second will explain the difficulties which I have encountered in attempting to bring the instrument to its desirable function as an aerial electroscope, and the point which I have attained in this new kind of meteorological observations. And in the third, I shall offer to the attention of the natural philosopher, some meteorological observations, which show the importance, in every branch of experimental philosophy, and especially in chemical theories, of forwarding the observation of atmospheric phenomena.

Part I.

On the Electric Column.

I have explained in the first paper, my hypothesis concerning the cause of a motion of the electric fluid, produced by the properties of two associated metals; and as all the circumstances attending this motion are characteristic of its cause, this connexion will be here my principal object.

I shall begin by some experiments previously mentioned, in which brass tripods were placed between the two metals, and these groups separated by pieces of wet cloth. In this first dissection of the pile, both electric and chemical effects were

produced, the latter on account of the water contained in the cloth; and I wanted to know what would be the consequence, in the same arrangement, with respect to the electric effects of suppressing the chemical ones, by a dry intermediary substance. For this purpose I substituted for the silver plates and the wet cloth of the former experiment, pieces of Dutch gilt paper, placing the brass tripods between the zinc plates and the copper side of the papers; and after having found, that forty such groups could be contained in each column of the frame described in that paper, forming in the whole eighty groups, I made the following experiments.

Experiment 1. In order to judge what would be the effect of interposing the brass tripods between the metals, I first mounted the eighty groups without them, and observed the degree of divergence of the gold leaves at both extremities. Dry paper not being so good a conductor as wet cloth, the electric effects were not so great as they would have been with the latter; but they were sufficient for my purpose. I kept this apparatus for some days, observing the divergences in different parts of the day, and the greatest, which happened to be at the zinc side, was of 0·3 of an inch.

Exp. 2. I then placed the brass tripods between each zinc plate and the copper side of the papers, the paper side of which separated the binary groups of metals, as did the wet cloth in the former experiment: the electric effects remained the same as they were in Exp. 1, without the tripods.

It came then into my mind, that the tripods, being of brass, might alone produce some effect, with only plain paper to separate these groups of zinc and brass; but the latter having but a small mass, the transmission of electric fluid along the column could not but be slower; and this was the very reason which determined me to the trial, as an experiment that would also relate to the cause which renders the column an aerial electroscope: this cause is the action of the ambient air, the immediate effects of which are, to lessen the positive state of one of the extremities, and the negative state of the other, according to its own electric state; and more slowness in the motion of the fluid giving more time to this action of the ambient air for diminishing the electric indications at the extremities of the column, these symptoms were to be smaller. This therefore induced me to make the following experiment.

Exp. 3. I mounted the column with eighty groups composed of zinc plates with only the brass tripods, separating them with pieces of writing paper, and I kept also this column for some days, observing the electroscopes at its extremities: they had the same variations which I had before observed, but very small, and the greatest divergence, which, as it is commonly, was in the middle of the day, did not exceed 0·1 of an inch.

I now come to that slowness mentioned above in the motion of the electric fluid produced by the property of the column, which

being attributed to a fluid known to possess, in the proper sense, the swiftness of lightning, must appear a paradox. Rapidity of motion certainly belongs to the electric fluid, when darting in a torrent; but the electric matter, of which it is composed, has a tendency to adhere to all bodies, air included; and it is this very property, as explained by Sig. Volta, which occasions the electric motions, when the fluid, tending to move by a rupture of its equilibrium, is more reluctant to be separated from the body which possesses it, than the latter to follow the fluid in its motion.

The effect of this tendency of the electric fluid towards bodies, in retarding its motion within the column, is analagous to an effect observed in water. When water is kept in motion in a channel by a constant supply, it is seen to take its course in the middle, leaving behind the particles retarded by their tendency towards the sides, which is decreasing as the distance increases. But the analogy is more direct, when water is confined in a pond, beset with aquatic plants or other impediments; for the motions impressed on that water at one side of the pond, though continued, if small, are but slowly and seldom completely communicated through the whole space. The case is the same with respect to the electric fluid set in motion by the property of the column, not only when confined within it, but when a current is produced; which effect will be shown by some experiments, after I shall have explained some parts of the figure annexed to this paper. See Plate I.*

The dimensions of this figure are half those of the original: it consists of many parts which I shall successively describe, beginning by those which relate to my present object; its fundamental parts are an electric column, with its electroscopes. The former is represented at A, B, supported horizontally on two pillars 1, 1, consisting of solid glass rods, covered with sealing wax, or with some other insulating varnish, and fastened on the wooden base 2, 2, by female screws underneath. The column itself is composed of 600 groups, formed of zinc plates 0·7 inch diameter, and equal pieces of plain Dutch gilt paper; the copper side of which being turned towards A, this is the positive extremity of the column; and as also, in every group, the paper itself serves only to separate the binary groups of zinc and copper, the latter being in each of them on the side of B, this is the negative extremity. The groups are contained between three glass rods, covered with sealing wax melted over them while hot, and fixed in holes of the brass plates A, B, where they have been introduced while the plates were hot, and the holes filled with sealing wax. These brass plates have in their lower part a pin, which enters freely into the brass cap at the top of the pillars 1, 1. At the extremities of the column are screws 3, 3, formed on the outside in the shape of loops; they serve first, to press the groups between the glass rods; and besides to produce, by brass wires hooked in their loops, the communication of each extremity with the nearest electroscope, as represented in the figure.

* Plate I could not be got ready in time to appear with this number.—Edit.

In general this column produces too great effects for the experiments which I have here in view, as the gold leaves strike the sides of the electroscopes; while there should be merely a simple divergence: therefore, either a smaller column must be used, or the time must be chosen when the 600 groups produce only this effect.

Exp. 4. Having observed the actual quantity of divergence in both electroscopes, when I lay my finger on the top of either of them, in order to produce the communication of its extremity of the column with the ground, the divergence ceases in it, and becomes nearly double in the other. Then taking off my finger, and thus abandoning the column to its own operation, the divergences are not immediately restored to their former quantity; it requires some time to produce them again, even half an hour or an hour.

This shows a reluctance in the parts once possessed of the electric fluid, to obey the cause which requires more of it on zinc than on copper, in order to establish the equilibrium between them. But this concerns only the quantity of electric fluid, which the column possesses in common with the surrounding bodies and the ambient air; for at the same time that this quantity delays to obey the law of the column, if an insulated body, either positive or negative, but in a degree which can merely affect the gold leaf electroscope, be applied to one extremity, the effect is instantly perceived at the other. This again is the same as if a stream of water were introduced at one side of the pond of the above example, and an opening made at the opposite side; for a current would be directly produced through the pond.

Exp. 5. After having disturbed the state of the divergences at the extremities of the column, by placing a finger on one of them, the mode of reproducing speedily the former state is, to lay the fingers on both extremities together, and remove them at the same instant: if this last condition be really obtained (which is not easy) the original divergences are restored.

These experiments cannot leave any doubt, that the phenomena of the column, as well as the electric part of those of the pile, are produced by a fluid set in motion.

Exp. 6. In the same case as that of the above experiments, namely, when the divergences are not too great, if one side of the column be placed in communication with the ground, the effects of the contact on the other side are so similar to those produced in the same manner on the leaves of the *mimosa sensitiva*, that this conformity of effects seems to indicate some analogy between the causes: both contacts make the leaves fall; they rise again, but it requires some time. There is an objection against the idea that the phenomenon of the plant is electric; because in the column the same effect is produced, at one side by imparting, and at the other by taking off, some electric fluid. This objection however is not absolute, for we do not know all the actions of organic bodies on the electric fluid; but if it is not this, it must be some other fluid,

which is acted upon by the contact of the plant. In general, we are very little advanced in the knowledge of the subtile agents operating in terrestrial phenomena; and as we cannot make any real progress in this knowledge but by endeavouring to increase, by observation and experiments, the number of the phenomena which have analogies with each other in some respect, it might be useful to follow an attentive comparison at different times and in different arrangements of circumstances, between the effects of contact on the gold leaves of the column, and on the leaves of the *mimosa sensativa*, and even contact with different bodies.

I come to greater symptoms of the motion of the electric fluid in the column, beginning by experiments, which will prove what, in the former paper, I have concluded, from my theory, namely, that by the cause assigned to this motion, the negative effect goes on increasing from the zinc to the copper extremity of the column, at the same time that the positive effect increases from the latter to the former; and that the electric state of each point of the insulated column is the sum of the correspondent terms of two inverse series of progress represented by determined, though in some respects variable, numbers, in a table given in that paper.

For these experiments a third electroscope is used: in the figure it is seen connected with the middle point C of the column, where is a thick brass plate with a projecting loop, 4. This immediate connexion of the electroscope with the middle point of the column serves for some experiments; but every other point of the column may be made to communicate with it, by the interposition of a soft wire held in the middle by an insulating handle. When this is used, the communication of the electroscope with the middle point is taken off; and, by bending the wire, it is easily made to connect, as conductor, the necessary parts of the column with this electroscope.

Exp. 7. At a time when there are simple divergences in the electroscopes at the extremities of the column, if they be equal, positive at A, and negative at B, there is no divergence in the electroscope at C; this is neutral, which is the case expressed in Table I, of the former paper;* and if at this time any point of the column, at a distance from the point C, on the negative or positive side, proportional to one of the terms of the table, be tried with the insulated conductor, the divergence which it produces will be found, as exactly as can be expected in such experiments, correspondent to the number expressed in the table, with its sign.

Exp. 8. In this situation of the column the states expressed in Table II, and Table III,† may be also observed by placing alternately its extremities in communication with the ground; but by a wire, because the metallic chains, commonly used for this purpose, do not transmit completely such small quantities of electric fluid, probably on account of some dust getting between the links. The following are the two cases of this experiment.

1. When the communication with the ground is made at B, the

* Volume viii, p. 449. † Ibid.

electroscope at the middle point C diverges positively, in the same degree as did before the electroscope at A; and the divergence of the latter is now nearly double. I say nearly, because equal increases in the electric state produce smaller increases in the angular motion of the gold leaves in proportion as the angle increases. By using then the small insulated conductor, it is found, that the whole column, (except the very extremity B, which, communicating with the ground, is neutral) is in a positive state, increasing towards A: which is expressed in Table II.

2. When the communication with the ground is made at A, the electric states of the column are all reversed. The electroscope at the middle point C has now a negative divergence, equal to that of the electroscope at B in the insulated state of the column; and the divergence at B is nearly double. Then, by observing the state of the other parts of the column with the insulated conductor, the negative state is found increasing toward B, from A, the only point not negative, but neutral. This is the case represented in Table III.

In the three different cases above described, the indicated positive and negative states are, in every part of the column, common to zinc and copper. There is no doubt, in every binary association of the metals, that difference between them which their nature requires; but it is insensible in them individually, as it is when they are single; and their electric state, embracing both metals, is determined, according to their position in the column, by the motions of the electric fluid resulting from these insensible elements; and that they follow the laws determined in my paper from the cause assigned, is verified by these experiments; which demonstrate at the same time, that there are no positive or negative states belonging to any part of the column (nor consequently of the pile): since each part may change from positive to negative, or inversely, according to circumstances, by the different motions impressed on the electric fluid; which motions may be concluded from these phenomena themselves, but will be directly perceived in the following experiment.

Exp. 9. A necessary condition of this class of experiments is, that the state of the ambient air be such, that alternately, at each extremity of the column, one of the gold leaves strikes the side of the electroscope, and at last sticks at one of the extremities. The following are three different cases in these phenomena.

1. When the strikings are alternate at the extremities, these instantaneous communications of the column with the ground at each side, by the contact of the gold leaf with the tin foil, produce in the former a flux and reflux of electric fluid. When the gold leaf strikes at the copper extremity, some fluid ascends from the ground into the column, and repairs the deficiency on this side; but this additional quantity of electric fluid in the column occasioning a striking at the zinc extremity, the new quantity of fluid returns that way to the ground. These rises and ebbs of the electric fluid in the column

are observed at the middle electroscope, but only when some time elapses between the strikings; for, on account of the slowness of motion of the fluid, directly shown above, when the strikings rapidly alternate, before one of the effects has extended itself in the column, the contrary effect begins; in the same manner as the rise and ebb of the water are not sensible within the Mediterranean Sea and the Baltic, on account of their narrow entrance; for before the flux has extended itself some way up these seas, the reflux operates in a contrary direction. But these motions of the electric fluid are very sensible at the middle point, in the following cases.

2. When the gold leaf comes to adhere at the copper extremity, thus placing it in a continued communication with the ground, the strikings, which become more frequent at the zinc side, produce a pulselike stream of the electric fluid in the column, manifested by the motions of the gold leaves in the middle electroscope: their divergence is positive, the whole column being now in this state (Exp. 8, 1), but they fall in part, when the gold leaf strikes at the zinc side, and rise in the intervals of the strikings; thus pointing out clearly a current flowing from B to A, at a higher level than the standard, which level alternately rises and falls.

3. When the adhesion of the gold leaf takes place at A, the zinc side; which circumstance, producing a continued communication of this side with the ground, renders the column negative in the whole (Exp. 8, 2); while the gold leaf at B, the copper side, goes on striking: a current of electric fluid is also produced, but at a lower level than the standard: the divergence of the gold leaves in the middle electroscope is consequently negative, and, as in the former case, they fall in part at every striking, and rise in the intervals; but while in that case they fell by the lowering of level of the current, and rose when it came higher; now diverging as negative, they fall at every striking, because some fluid, ascending from the ground, makes the column less negative; and they rise again while this fluid flows into the ground by the zinc side, and thus prepares another striking.

I have made the same experiments on the motions of the electric fluid within the pile itself; they are more confused and less lasting on account of the calcination of the metals: but the column, being in fact the electric machine of the pile, shows clearly and permanently these motions, which I shall now follow in the circuit, or when the two extremities of the column (or the pile) are connected together by some intermediate body. In this case the motion of the electric fluid is manifested by more or less retardation of its current, according to the degree of conducting faculty of the body employed.

For this class of experiments (see the figure) brass hooks, 5 and 6, are fixed to the small brass plates, terminating the column at each extremity, and against which press the screws: these hooks project a little more than an inch, and serve for different purposes. The

following experiments will relate to the conducting faculty of that kind of glass tube filled with water, entered by wires on both sides, in which chemical effects are produced when it is applied to the pile; but with the column, these effects do not take place.

Exp. 10. The tube of the above kind, which serves in this experiment, is represented in the figure, as suspended at the point 7; its wire, 8, having a hook, held up by a silk thread, which, passing over the pulley, 10, descends to a thin brass plate, 11, fixed to the base of the instrument. This brass piece bends forward at the top, and the silk, entering into a notch of this projection, is stopped there by a bead fixed to it. The other wire, 9, of the tube, is hooked on the projecting wire, 6. In this situation of the tube, it does not affect the electroscopes of the column, they continue to diverge as if the tube were not connected with one of them; but when, the silk being disengaged, the hook of the wire, 8, comes to rest on the hook, 5, of the column, the circulation of the electric fluid produced through it between the extremities A and B is so rapid, that the divergence entirely ceases in the electroscopes; and it returns only, when the extremity of the wire, 8, is again lifted up. This shows, that the glass tubes of this kind are sensibly as good conductors as metals.

The different conducting faculties of bodies proceed from different degrees of adhesion of the electric fluid to them: but beside this difference among bodies, there is another, which relates to permeability. All the bodies, which I have tried, are permeable to the whole of the electric fluid, except those that can be charged; which are impermeable to the electric matter, and permeable only to the vector. This operation, called charge, as I have explained and proved in my works, consists in accumulating the electric matter only on one side of the laminæ made of these substances, by occasioning a proportional diminution of its quantity on the opposite surface, which is an operation of the vector; and the reason why other bodies cannot be charged is, that, the electric matter pervading them, though slowly in some of them, no sensible difference in the quantity of electric matter can be produced between their opposite surfaces.

Glass, in this respect, is a remarkable substance. It is absolutely impermeable to the electric matter, and, being a solid body, it is used for insulating pillars in our electric apparatusses; but the electric matter moves easily along its surface, as I have visibly shown by the Lichtenberg figures produced on its naked surface, where they dissipate in a little time, while they last many days on resinous surfaces. This is the reason of covering the glass pillars destined to insulate with some resinous varnish; but all these varnishes are not equally fit for the purpose, and this is one of the objects of the following process, as well as the trial of the different conducting faculties of other bodies.

For these experiments, the bodies to be tried must be reduced

into slips or rods, which are to be laid on the hooks, 5 and 6, of the column, in order to observe the effect produced on its electroscopes; but there are necessary precautions to be used in laying them on the hooks. For instance, in respect to the bodies with which I shall begin, those which have different conducting faculties, belonging mostly to the vegetable and animal kinds, when they are placed on the hooks with the fingers, as it is almost impossible to lay them on both hooks at the same instant, the end which touches first disturbs the equilbrium of the electric fluid in the column; and I have shewn above, that it is but slowly restored. In order to obviate this defect, and for another purpose that will follow, two brass wire brackets, 12, 12, are fixed in the front of the base of the instrument, on which the slips are first laid, and there taken up by two glass hooks covered with sealing wax, with which they are placed on the hooks of the column. I shall give a general idea of these trials under the following head.

Exp. 11. The substances of this class having more or less conducting faculty, they lessen in different degrees the divergence in the electroscopes, by transmitting more or less electric fluid from A to B. This is a curious kind of experiment, but as the particulars are not here my object, I shall relate only one, concerning the physiology of vegetables, which may lead to others of the same kind. Having repeated in presence of Dr. Lind these experiments on the different conducting faculties of various bodies, I showed him a phenomenon, which had surprised me. A thin slip of deal, cut along the fibres, being applied to the column, there remained but little divergence in the electroscopes; while a slip of the same wood, of the same thickness and breadth, but cut across the fibres, produced much less diminution in the divergence. Dr. Lind found probably the cause of this. difference, assigning it to the situation of the resinous substance within that wood: it does not belong to the fibres themselves, since they transmit easily the electric fluid; it is lodged between the fibres, and thus forms an impediment to the passage of the fluid from fibre to fibre in the slips cut across them.

When these experiments are made with the view of trying the insulating property of bodies, still more precaution is required in placing them on the hooks of the column: for the bodies fit for this use being fundamentally impermeable to the electric matter, their electric state is changed, more or less permanently, by friction; and this in the manner which I intend to explain in a future paper. As, however, they can hardly be handled without some friction, they act upon the column by their influence (an effect which I shall show directly hereafter), and their insulating property cannot be observed, on account of the disturbance which they produce in the state of the column itself. These bodies therefore must remain a little time untouched on the brackets, and be there breathed upon, in order that the moisture of the breath may dissipate their electrization; serving as a conductor for their whole surface to the ground, through the

brackets; and when the moisture is evaporated, the rods are taken up there with the insulating hooks, and thus applied to the column.

These experiments are particularly useful for a better knowledge of insulation, a point very important in the construction of electric apparatusses; for many experiments fail for want of a complete insulation; and I do not know of any shorter and surer method of trying the isulating faculty of varnishes laid on glass for this purpose, than that of applying to the column rods of glass covered with them. I shall also give only a general idea of this class of trials under the following head.

Exp. 12. It is very seldom, that a naked glass rod, being placed on the hooks of the column, does not sensibly diminish, in a little time, the divergence in the electroscopes, by transmitting slowly some electric fluid from A to B: but this is more or less, according to the nature of the glass; therefore these differences may become a particular object of experiments concerning the property of different glasses for electric purposes, by comparing the effects of different rods on the electroscopes of the column; much care being taken, that all effect of friction be dissipated. With respect to varnishes, a proper sealing wax laid over the glass when hot is the best coating which I have found; a rod of this kind produces no change in the electroscopes. But all sealing waxes have not the same property, and before I had devised this mode of trial, I was sometimes disappointed in the construction of the small set of electric apparatusses, which I have mentioned in the former paper, even in that of the electroscopes of the column, as the top of their glass bottle must be insulating. The sealing wax reckoned the finest, because it melts more easily and spreads more smoothly, is not fit for this purpose, its softness being produced by spirits of wine. In general, for this essential choice of an insulating coating on glass, the column is very useful; for by laying the different coatings on glass rods, and placing these on the hooks of the column, those which will be found to diminish the divergence in the electroscopes are not completely insulating, and that coating must be used, by which the divergences are not affected.

I come to the impression produced on the column itself, when there remains some effect of friction on the insulating rods applied to it. The experiments on this object will, at the same time, afford a new verification of the cause which I have assigned to the motion of the electric fluid in the column (or the pile), by what is called the electric influence; the laws of which, first really determined by Sig. Volta, I have explained by some modifications of the vector, which will be perceived in the following experiments; showing at the same time the effect of the ambient air, such as I have determined it. The proper time for these experiments is also when, from the electric state of the ambient air, there is not much divergence in the electroscopes of the column. They may be made with naked glass, which friction renders positive; and with a glass rod covered with sealing

wax, which thus becomes negative. I shall explain the phenomena produced by the latter, because it retains longer the effect of friction; those produced by naked glass are of the same nature, only reversed.

Exp. 13. A glass rod covered with sealing wax must be rubbed so gently, that, when applied to the column, it only increases the divergence of the gold leaves at the negative side, without any striking, else the effects would be confused: when it produces the proper effect, the phenomena are the following.

1. At the moment when the rod is placed, the divergence increases at the negative side, and diminishes, or even ceases at the positive side. The cause of these effects is, that the expansive power of the electric fluid is lessened in the whole column, by part of its vector passing to the negative rod, where its quantity is less: and as however the equilibrium of the electric fluid in the column requires more electric matter on zinc than on copper, the latter, at the first moment, loses more of it, but not sufficiently to compensate the loss of expansive power at the zinc side; therefore less electric fluid passes into the gold leaves of the latter, which fall in consequence of this diminution.

2. Within a little time, the positive divergence is renewed at the zinc side, and the negative lessened at the copper side. This effect is produced by the ambient air, which during the diminution of expansive power in the column, yields to it some electric fluid, especially to the copper side which was become strongly negative; and thus the former equilibrium is restored in the column.

The proof of the above explanations of the phenomena observed, which embrace the whole system, both of the motion of the electric fluid in the column, and of the influence of the ambient air on this instrument, is obtained by suddenly moving the rod: for the new quantity of electric fluid communicated to the column by the air during the influence of the negative body makes both gold leaves strike at once on the zinc side by a very great divergence.

The last object concerning the theory of the electric effects in both the column and the pile, which remains to be considered, relates to the difference between the effects of the number of groups and the size of the plates, considered under two distinct points of view, on one of which I have already given an experiment in my former paper: but I have repeated it in a different manner, which will confirm my system on these phenomena.

Exp. 14. The column of 600 groups, represented in the figure, has been composed of three columns of 200 groups each, which I had used separately from the beginning of these experiments; but before they were united together for the purpose of the aerial electroscope, I tried their effects in the three following combinations.

1. I applied successively to the same electroscope the same extremity (either positive or negative,) of each column, the opposite extremity being placed in communication with the ground; and I observed the quantity of divergence produced by each column, which was nearly the same.

2. I applied the three columns at the same time to the electroscope, each of them remaining in communication with the ground; and the divergence was not greater than it had been with the most active of the single columns.

3. But having connected the three columns as one, by placing conductors between their opposite extremities, and connecting one extremity of the whole with the electroscope, the other being in communication with the ground; the effect was so much increased that the gold leaves struck the sides of the electroscope.

This proves, under a different form, the same proposition which I had stated in my former paper, namely, that the simple divergence in electroscopes depends only on the density of the electric fluid, and the density on the number of groups; at the same time that it confirms the cause which I had assigned to these effects; and as they are analogous to many kinds of phenomena, I shall use another example to explain it, that of pumps. As the height to which water can be raised by pumps does not depend on either their number or size, but on their length; so in the above experiment, with three concurrent columns of 200 groups, the density of the electric fluid was not increased on one extremity, nor consequently the divergence at either extremity, more than with one column; nor could more have been done with one column of the same number of groups of whatever size. But as, in taking the water at the same level, a pump of thirty feet will bring it three times as high as three pumps of ten feet; so in the above experiment, the column of 600 groups produced probably three times as much change of density in the electric fluid, with a proportional divergence, as did the three columns of 200 groups, individually acting on the same low level, or degree of density, that of the standard of *plus* or *minus*.

But if in this case the size of the plates, or the multiplication of their number at the same numerical distances from the extremities, be indifferent, it is not the same in some other cases, as I shall illustrate by the same analogy. In the above case, the height to which the water was to be raised being the only object, the number or size of the pumps was indifferent; but if a current is to be produced at that level, either with a certain degree of rapidity, or of a certain volume, then the diameter of the pumps comes in as a condition. The following experiment will show the analogy of this case, with the effects of the different sizes of plates in the column.

Exp. 15. I made two other columns of 200 groups each; but these I only cut square, for one of $\frac{1}{4}$, and for the other of $\frac{1}{8}$ of an inch, still zinc and Dutch gilt paper. These two columns produced sensibly the same divergence as the former, in the same electroscope; but in this was already shown the difference in other respects; the time for producing this divergence was in the inverse ratio of the size of the plates.

This experiment gives a clear idea of the effect produced by a greater size of the plates, both in the pile and in the column. In

the circuit of the former, with the same number of groups, the effects are proportional to the size of the plates, because the current of the electric fluid becoming denser and more rapid in passing through the wires used in these operations, the effects are greater, in proportion to the number of equal parts of surface, either in a few or many plates, concurring to produce the motion of the electric fluid which arrives at the entrance of this narrow channel. That difference in the rapidity and density of the current cannot be discovered in the circuit of the column, because the condition to which chemical effects are owing is wanting in it, as I have explained in my former paper; but the size of the plates influences the frequency of the strikings of the little electroscopic pendula, when their simple divergence is exceeded; because each time that one of them strikes, either at the negative or the positive side, that instantaneous communication of the column with the ground changes in some degree its electric state; and the same state is sooner restored, to produce another striking, in proportion to the size of the plates, with the number of groups. This effect will enter as an essential circumstance into the second part of this paper, concerning the aerial electroscope.

On the Ignition of Gunpowder by the Electrical Discharge; and on the Transmission of Electricity through Water, &c. By William Sturgeon.*

[Experimental and Theoretical Researches in Electricity, Magnetism, &c., continued from page 346, vol. viii.]

It is generally admitted that the present state of knowledge relative to the phenomena of electricity is enveloped in much obscurity; and perhaps no instance of electrical action manifests our ignorance of this branch of science more than that of igniting gunpowder. Yet so little notice is taken of this isolated fact, that no satisfactory attempt at explanation, that I am aware of, has ever appeared in the pages of any writer on this subject.

That gunpowder has frequently, by various individuals, been ignited by the electric fluid, is a truth that cannot be denied. But *why* those experimenters happened to succeed, and *why* others so frequently and still more constantly *fail*, are circumstances the cause of which has hitherto been left unexplained,—perhaps not understood. I am well convinced that no individual experimenter has been more embarrassed than myself, by fruitless attempts to ignite gunpowder by the electric fluid: and although I have varied the experiment according to all the directions I could either read or hear of, yet I candidly confess that I never succeeded by any of them.

Fruitless as these experiments were with respect to the object in view, it was observed, by passing the discharge of a jar through water (which is the method given by some authors), that the force of the shock is considerably abated; and that the report is very trifling when compared to that which is heard by a similar discharge

* From the *Phil. Mag.*

through metal. Convinced by this circumstance that the nature of the discharge is *modified* in a peculiar manner by passing the fluid through water ; yet, as I had never succeeded in igniting gunpowder by a discharge through the aqueous medium, it appeared evident that something more than this fact was necessary to be understood. I therefore became desirous to ascertain, if possible, the real cause why other experimenters succeeded, and why every attempt that I had made proved unsucceessful.

It is well known, that if a red hot iron be applied to gunpowder, the latter does not immediately ignite, but that some interval of time (however small), does certainly elapse before one single grain is on fire : and that it is possible for a red hot iron to be passed over the hand with such velocity as to produce scarcely any sensation of heat. Hence my first object, now, was to devise some means of retarding the velocity of the electric fluid : for I considered that if this could be acomplished, more time would be afforded for the fluid and gunpowder to be in contact, and the latter, in consequence, more likely to be ignited. I likewise supposed that if the electric fluid be retarded by being transmitted through water, it was likely that a jar would not be quite discharged by a very sudden contact ; as it was probable that if the discharging rod was quickly withdrawn from the knob of the jar, that the whole of the fluid would not have time to make its escape. But several trials in this way, through a large tub of water, seemed to discharge the jar as effectually as if the whole circuit had been of metal.

Although these experiments were by no means satisfactory, yet I always observed that the report was much feebler, and that gunpowder placed in the circuit was not blown or scattered to so great a distance when the discharge was made through water, as when a similar discharge was transmitted through metal. Hence it was obvious that the force had by some means abated ; but whether by retardation, or by some change in the physical character of the electric fluid, I was at that time unable to determine.

Some time afterwards an idea suggested, that if the electric fluid be retarded at all by passing through water, perhaps the water possesses this property in consequence of its inferior conducting capacity with respect to metals and other good conductors ; and if so the velocity of the electric fluid might be reduced to almost any degree, by reducing the diameter of the column of water through which it had to pass. For it is evident that the conducting power of any body will be proportional to its natural capacity, and to the quantity employed at any one point in the circuit. For a discharge that will destroy a thin wire, would be conducted with safety by a wire of the same kind of metal, of greater dimensions. It now occurred that those persons who had ignited gunpowder by the electric fluid, perhaps succeeded by using very narrow tubes filled with water. (For I had frequently transmitted a discharge through a wide tube without success ; and as no author gives any dimensions of the water

employed, it did not till now occur that the time of transmission would vary with the calibre of the tube.) I had not at this time any narrow tubes in my possession. Considering, however, that if any non-conducting substance—such as silk, or paper—were moistened with water, that those substances could have no more conducting power than what was imparted to them by the moisture; my first experiment was with a single thread of sewing silk, about four inches long, well moistened by drawing it between my lips. This thread was made a part of the circuit between the inside and the outside of a charged jar. At another part of the circuit an interruption was made between the extremities of two wires; and at this interruption was placed some gunpowder. On discharging the jar the gunpowder ignited. I repeated the experiment several times with the same success. I afterwards varied the experiment, by using the same thread and a smaller jar, and succeeded in igniting gunpowder with about thirty inches of charged surface. I must here observe that when the thread was very wet, I never succeeded with this small jar, owing, as I suppose, to the quantity of water contained in the thread being too great to retard the small quantity of electric fluid contained in the jar. For by squeezing out some of the moisture, the thread became a worse conductor, and then I always succeeded.

I next tried how far it was possible to succeed with the first jar, and augmenting the quantity of water. For this purpose a piece of twine was used, well soaked in water. This twine, however, conducted the electric fluid with too much facility to ignite the gunpowder; but when some of the moisture was squeezed out it answered very well. Thus by proportioning the one with the other, I always succeeded. The same results were obtained by using moistened paper.

Having satisfied myself on this point, I next endeavoured to ascertain if the electric fluid undergoes any change in its physical character by passing through water; or if the ignition of gunpowder depends entirely on the time occupied by the fluid to pass through it. For this purpose I employed two jars, which for distinction we will call A and B. I charged A positively and B negatively, and connected their outsides by water. On exploding A into B, through water, both jars became neutralized. A was again charged positively, and afterwards partly discharged through water into B. On discharging both jars separately there appeared no difference in the explosions. A was once more charged positively, and again partly discharged through water into B. On discharging B through a moistened thread, gunpowder was ignited in the circuit. These experiments were reversed by charging the jar A with negative instead of positive electricity, and the results were similar. Hence I concluded that the ignition of gunpowder by the electric fluid depends on the time occupied by the latter in passing through it, and not on any change in the physical character of the fluid.

Hence also, by the foregoing experiments (when the intensity of the charge is constant), the time occupied by any given quantity of the electric fluid, in passing any one point in the circuit, will be in some reciprocal proportion* to the thickness of the column of water employed in that circuit.

It was observed in these experiments, that the moistened thread soon became nearly dry. Hence the electric fluid had either decomposed the water or caused it to evaporate. The former effect I suppose to have taken place; which, if true, perhaps the decomposition of water by this agent may be facilitated by reducing the diameter of the column employed. I have not yet had time to ascertain this particular satisfactorily, therefore it remains a mere supposition.

I have frequently discharged a jar through my own body without any other inconvenience than a burning sensation at the extremities of the fingers; and have ignited gunpowder in the same circuit.

So modified is the electrical discharge by being transmitted through aqueous conductors that the effect of an intense charge of the most powerful battery may be reduced to almost any degree. I have discharged eight feet† of charged surface through my own body without feeling the least shock. But the burning sensation was very severe.

In medicine this modification of electricity can hardly fail to be useful; for it may be administered to any particular part of the body without affecting any other part. It may be applied to the skin of the most delicate patient; and, without the least danger of giving a shock, a most powerful stream of electricity may be poured on the part affected.

It has been also ascertained, that the force of the electric discharge through metal, is always proportioned to the thickness of the wire through which it is transmitted; or, that the same quantity and intensity of electric fluid acts with a greater force when transmitted through a thick than through a thin wire.

This law, of course, has a limit; for if the conducting wire be sufficiently stout or capacious to transmit the fluid without interruption, a wire of larger dimensions can give no more facility to the transmission. I am of opinion, however, that thick wire facilitates the transmission of the electric fluid to a greater degree than is generally suspected. Now it is evident that as thin wire has the property of diminishing the intensity of an electrical discharge, the fluid during its transmission through such a wire must necessarily be

* This proportion may vary either as the diameter, or as the square of the diameter of the column: according as the electric fluid occupies the surface, or the whole body of the water; and if the velocity of each individual particle of the electric fluid be affected, the transmission of the whole mass will also vary on that account.

† Eight feet of lining and eight of coating.

drawn out (as it were) into a longer stream, than if it were transmitted through a thick one. In the same manner that a certain quantity of water would be drawn out into a longer stream by passing through a narrow than through a wide tube; or as a piece of metal would be drawn into a longer wire by passing through a small than through a large hole in the plate.

Hence it became a curious question, What length of wire of a given dimension does a certain quantity of the electric fluid (say a jar charged to a certain intensity) occupy at any moment during its transmission? I supposed that this might probably be ascertained by placing some gunpowder at an interruption near to the positive side of the jar, and the moistened thread near to the negative side, having a long copper wire between them. For if the wire was of sufficient dimensions to contain all the fluid at once, the latter would not meet with any resistance till it arrived at the moistened thread; and the time of its transmission through that part of the circuit where the gunpowder was placed would not be lengthened; upon which supposition the gunpowder ought not to be ignited.

With such arrangements I have separated the gunpowder and moistened thread by a copper wire, No. 16, of different lengths, from one to twenty yards; yet with all this length of good conducting substance beyond the gunpowder, the latter never failed to be ignited.

I consider this a very curious circumstance, and the inquiry important. Not so much because the gunpowder always ignited in these experiments (for it is possible that with longer and stouter wire, and a smaller charge, the ignition might not take place); but, because, if it could be proved by experiment that the electric fluid would, by intervening capacious good conductors, ignite gunpowder at the negative, and not at the positive side of the moistened thread, such experiment would prove to demonstration the truth of the Franklinian hypothesis.

I am well convinced that if the electric fluid ever passed the gunpowder without interruption, the latter could not be ignited by any recoil of the fluid into the jar from the interrupting moistened thread; because if ever it passed through the gunpowder with violence, it would scatter, or blow the latter substance away, so that none would remain in the circuit to be ignited at the time of the fluid's return.

Several experiments on the ignition of gunpowder by the electric fluid were exhibited, and demonstrated on the foregoing principles, in a lecture which I had the honour to deliver before the members of the Western Literary and Scientific Institution, on Monday evening last (May 8th), in the Concert Room, King's Theatre, Haymarket. In one experiment the gunpowder was ignited at one extremity of the moistened thread; and in another experiment four guns were fired in the same circuit. I have frequently fired six

guns by one discharge of a jar; and so instantaneous is the ignition at the several guns, that their united reports appear like the report of one gun only.

I am at present engaged in other experiments on this branch of electricity, and shall not fail to communicate to the public the results of such as appear worthy of notice.

I am, Sir, your's faithfully,
W. Sturgeon.

Artillery Place, Woolwich,
May 15, 1826.

On the Inflammation of Gunpowder and other Substances by Electricity: with a Proposal to employ the Term Momentum *as expressive of a certain Condition of the Electric Fluid. By* Mr. W. Sturgeon.

To the Editor of the Philosophical Magazine and Journal.

Sir,—Since the publication of my paper on the ignition of gunpowder by the electric fluid, I have had an opportunity of perusing Mr. Woodward's paper* on the same subject. But I find that, like all other authors I have yet read, Mr. W. has left us entirely in the dark with respect to the *law* that is necessary to be observed in varying the experiment. For although the dimensions of his jar and tube are given, no mention whatever is made why it is necessary to observe those dimensions, or why other jars and tubes of various other dimensions may not answer as well. Indeed, so little does Mr. W.'s hypothesis concur with the conclusions deduced from my experiments, that he supposes the water derives its retarding property solely from its confinement in tubes. For it is stated in Mr. W.'s paper: "If the tube be filled with ether or alcohol, and placed in the circuit, the powder will be inflamed. If it be filled with sulphuric acid, which is a better conductor, the powder will be scattered and not inflamed: but the dispersion will not be so great as when metals only form the circuit.

"The same effect will be produced by transmitting the charge through the animal economy, or through water not inclosed in tubes, in which case the water does not appear to oppose a sufficient resistance to the passage of the fluid."

It is evident from this statement of Mr. Woodward's, that he never varied the experiment by employing tubes of various diameters; his experiments being with different fluids in the same tube. Had Mr. W. taken into consideration the necessity of varying the

* This paper is one of those you so obligingly pointed out to me for perusal; and appears at page 283, vol. vii, of the new series of the *Annals of Philosophy*. The two papers in vol. viii, contain nothing material on the subject.

diameter of the tube with the nature of the charge, I suspect he would have found little difficulty in igniting gunpowder by transmitting the electric fluid through sulphuric acid, it being necessary to observe nothing more than to augment the charge, or reduce the diameter of the tube.

That the retarding property of the water does not depend on its confinement, is evident from the success of my experiments with a moistened thread exposed to the open air. Indeed, so certain it is, that confinement does not impart to water this property, that gunpowder may be ignited in the electrical circuit, by transmitting the fluid through a sufficiently narrow streak of water, drawn on the surface of a piece of flat glass.

I should, however, be extremely sorry to detract any thing from the merits of any man; and although Mr. Woodward's explanation of igniting gunpowder by the electric fluid, when transmitted through water, does not exactly agree with the principles on which I suppose the action to depend, I nevertheless should be wanting in candour, were I not to express my sentiments on the highly interesting nature of his experiments: and I feel a pleasure in acknowledging that Mr. W. has preceded me in some that are detailed in my paper (although I did not know it at the time); viz., those wherein gunpowder was ignited on both the positive and negative side of a long conducting wire.

Mr. Leuthwaite has likewise conducted a number of interesting experiments on this subject; but I believe they were intended chiefly to ascertain the conducting power of different fluids, in order to make choice of the most eligible for the purpose of igniting gunpowder by the electric fluid.

Hence, so far it appears that the idea had not been entertained (prior to the institution of my experiments), that the diameter of the tube, and the nature of the charge, would any way affect the result of the experiment. Neither does it appear that the ignition of gunpowder, by means of transmitting the electric fluid through unconfined water, was ever attempted prior to those experiments detailed in my former paper on this subject. The most extraordinary method of effecting the ignition of gunpowder by the electric fluid, that I have yet heard of, is that stated by Mr. Howldy, in the *Philosophical Magazine*, vol. lxviii, p. 173. I have been induced to pay some attention to this method, "especially as it saves the experimenter time, labour, and power," circumstances highly important and necessary to be understood.

I think, however, it is to be regretted, that Mr. Howldy has not mentioned the hygrometrical state of that part of the table ("four inches") between the extremity of the chain and the outside of the jar: as it is possible, that a variation in that particular may vary the result of the experiment. But as Mr. H. has practised this method for several years, and with uniform success, it is to be expected that such a circumstance could hardly escape the notice of so accurate an

experimenter. Considering, however, that four inches is a long striking distance through dry air, and not happening to be successful when attempting to repeat the experiment according to Mr. H.'s directions, I have been induced to suggest to that gentleman the necessity of his repeating the experiment, under the following circumstances:

First. Let the table on which the jar and chain are placed be perfectly dry, and of hard wood (say an old mahogany table that has frequently been rubbed with bees'-wax, or with anything else to render it a nonconductor). Let this jar of 160 square inches of (interior) coated surface, be charged to the intensity of 80° per quadrant electrometer; every other part of the circuit being as he has described it. Discharge the jar through this circuit.

Secondly. Let the same arrangement again be made, only with this difference: draw on the table a narrow line of water (four inches long), from the outside of the jar to the extremity of the chain. Discharge the jar through this circuit.

Should there occur different results in those experiments (as I am persuaded there will), why did not Mr. Howldy mention such essential particulars? Or are we to conclude from his silence, that he entertained just notions of the nature of the experiment, and that this circumstance was too trivial to notice?

With respect to the method in which the wooden peg is used, I am aware that it will answer very well if regulated upon the principles stated in my paper: that is, when the moisture it contains in a transverse section is proportioned to the charge transmitted; and that, whether the point be sharp or blunt, and in whatever part of the circuit it may be placed. If its being "very dry" were essential to the success of the experiment, it appears strange, that when it possessed that qualitity in a superior degree, by "two or three experiments," it should be "rendered useless." Its having a sharp point, too, may possibly be the means of its possessing properties which I have not seen. * * * * *

I should, however, recommend those persons who use a piece of wood, to have it somewhat longer than that described by Mr. Howldy, for fear of meeting similar disappointments to those which he met with when varying the distance between the outside of the jar and the extremity of the chain with high intensities; as it is possible that the peg may be shorter than the striking distance.

Whenever an electrical discharge is transmitted through water, for the purpose of igniting gunpowder, the length of the aqueous column ought always to exceed the striking distance; for if the wires which enter the extremities of the column are brought too near each other, the electric fluid will dart from one to the other with very little interruption, and in all probability will scatter rather than ignite the gunpowder.

To insure success, the column or train of water, or any substance

containing it, such as wood, twine, silk, paper, &c., should never be shorter than six inches; and the thickness or quantity of water contained in a transverse section must always be regulated according to the nature of the charge; that is, to the quantity and intensity of the electric fluid employed. When a small jar is used for this purpose, then the strip of water must be very thin, or narrow: if a larger jar be used, the thickness of the aqueous column may be increased. When the same jar is used with different intensities, then the lowest intensity requires the thinnest strip of water; not because the intensity is lower, but because the quantity of fluid is less: and although a column of water that answers for a low intensity will answer for every intensity that is higher, nevertheless a column that would answer very well for high intensity, might be far too large to insure success with an intensity that is very low.

As glass tubes are both convenient and elegant for regulating the diameter of a column of water; about seven or eight inches of barometer tube, $\frac{1}{10}$ of an inch diameter, answers the purpose very well, with any jar containing more than half a square foot of coated surface on each side, with any intensity of charge above 50° per quadrant electrometer.*

By employing a glass tube of the above dimensions filled with water, and a jar containing 120 square inches of coated surface on each side of the glass, gunpowder was ignited in the circuit at every trial, with any intensity above 20°. When two such jars were employed at the same time, gunpowder ignited with every intensity above 10°; sometimes with an intensity as low as 7° or 8°. The gunpowder was fine grained, and of the best quality. If the gunpowder is bruised to a fine powder, it will answer still better.

When moistened thread, twine, or wood is used, any of them may be cut in two, and separated a little, and the gunpowder will take fire at this interruption of the watery part of the circuit. Thus we learn that it is not necessary for metallic conductors to be in contact with the gunpowder to insure its ignition by the electric fluid.

A chain may be made of alternate links of copper wire and tailor's thread, in such a manner as not to be easily distinguished from one that is all of metal. If such a chain be dipped in water, and made a part of the electrical circuit, gunpowder may be ignited in any part of that circuit, as though the thread were one continued piece.

When one of the above-mentioned jars was employed, and a copper conductor formed the circuit, ether was fired at an interruption, with every intensity above 20°. When the water tube formed a part of the circuit, ether could not be fired, though both jars were employed together, with an intensity of 90°.

* As quadrant electrometers do not afford uniform measures of electrical intensity, but differ from one another according to the weight of the ball, delicacy of suspension, &c., an intensity is here given, which, it is expected, will answer with the generality of them.

When four such jars were charged to the highest possible intensity, and discharged through the water tube, gold leaf placed in the circuit was powerfully attracted, but not deflagrated.

One jar with an intensity of 60°, powerfully magnetized a sewing needle, placed in a spiral forming part of a complete metallic circuit. With two jars charged to the highest possible intensity, no magnetic effect was produced when the tube of water formed a part of the circuit.

How do these experiments accord with the doctrine of *quantity* and *intensity?* In the galvanic process, a needle may be powerfully magnetized by a single pair of small copper and zinc plates; by which process, it is said, we have *quantity*, but not *intensity*. But by the experiments I have just now mentioned, it appears that *quantity* has not the power of magnetizing needles, without *intensity*. For when the electric fluid was not retarded by inferior conductors, a very small *quantity* produced the effect; but when the *intensity* of the discharge was reduced, although three times the former *quantity*, at least, was employed, not the slightest magnetic effect was produced. The truth is, we want another and a more appropriate term in electricity: that term is *momentum*. *Intensity* answers very well to express the relative degrees of concentration on the surface of jars, &c., but *momentum* is the proper term when the fluid is in motion. Hence, then, although the less quantity of the fluid produced on the needle an effect which the greater was not able to accomplish, this effect was probably owing to the *momentum* of the former exceeding that of the latter.

When tow or cotton is moistened with spirit of turpentine, or mixed with powdered rosin, it will ignite by a very small electrical discharge when the whole circuit is of metal. But if the tube of water form a part of the circuit, the tow, or cotton, prepared as above, will not ignite, although two of the before-mentioned jars charged to the highest possible intensity be discharged through the circuit.

When a narrow slip of gold leaf was placed between two pieces of glass, and made to form a part of a complete metallic circuit, one jar with an intensity of 8° discharged through that circuit, completely exploded the gold leaf. When two jars were employed at the same time, and charged to a still higher intensity than the former, and the water tube entered into the circuit, a similar slip of gold leaf subjected to the discharge, remained uninjured.

When gold leaf and gunpowder were subjected at the same time to a discharge similar to the above, and the whole circuit of metal, the former was completely exploded, but the latter substance was scattered only. When the water tube formed a part of the circuit, every other part arranged as before, the gunpowder ignited, but the gold leaf was undisturbed by the discharge of the jars.

Similar experiments were made with gunpowder and needles, gunpowder and ether, gunpowder and tow, prepared as above.

When the circuit was completely metallic, the needles were magnetized, or the ether, the tow, &c., were fired; but the gunpowder was in no instance ignited. When the water tube formed a part of the circuit, the gunpowder was, in every case, ignited; but the other substances remained unaffected.

Hence we may conclude, that in order to magnetize pieces of steel, to explode metals, to ignite ether, or tow, with resin, &c., by electricity, *quantity* and *velocity*, or *momentum* of the fluid is required; but to ignite gunpowder, *quantity* and *time* are indispensable: that is, when the quantity is constant. To produce the former effects requires *velocity*, to produce the latter effect, *time*.

I remain, Sir,
Your obedient servant,
W. Sturgeon.

Artillery Place, Woolwich,
Nov. 24th, 1826.

ELEMENTARY LECTURES IN ELECTRICITY, etc.

LECTURE XVIII.

In persuing electrical enquiries from the productions of the machine to those of nature, we find every principle that is exhibited from the one source, corresponds with those displayed from the other: from the simple attractions and repulsions, to the most magnificent phenomena producible by electric action. The air surrounding this globe of earth being intermixed with the electric fluid, as decidedly as the materials of the earth itself are charged with that subtle agent, it is reasonable to suppose that electro-fluctuations take place in the atmosphere as well as in the earth, by every change of temperature, hydrometric condition, &c., to which it is exposed; and as these changes are frequent, rapid, and occasionally great, they are attended by corresponding sudden, and great changes in the electric condition of the aerial shell.

It is a remarkable fact that, in a still, cloudless atmosphere, the electric condition of it, as high as has been explored, is that of a gradually increasing charge, from the earth's surface upwards; and as this is uniformly the case, under these circumstances, it becomes an establised fact in atmospheric electricity, which may be conveniently employed as a standard, or normal condition, in studies of this subject.

The ratio of electric increase with increasing atmospheric altitudes having never yet been determined, we are not in possession of any definite law; and as different results have been obtained by different observers, under circumstances apparently similar, the data hitherto collected are too uncertain, and too scanty, to form any basis whereon mathematical assistance could be available in the establishment of any correct law on this interesting topic. Notwithstanding, however, the want of precise theoretical laws on

the electro-distribution in the atmosphere, being an acknowledged fact, the desideratum is pleasingly, though partially supplied to the observer, by the unfading picture which is formed in his mind, of the certain increase of electric agency at increasing altitudes from the earth's surface; every succeeding stratum being electro-positive to every stratum beneath it; and the whole atmosphere electro-positive to the globe which it surrounds.

The easiest and surest mode of ascertaining the different electric conditions of the atmosphere at different altitudes, is by means of a series of kites, with a wire strand in each string

The kites, which ought to be four or five in number, are to be floated at the same time, and with different lengths of string, from a hundred to a thousand or more yards. Let, for instance, the lowest of a series of five kites have only one hundred yards of string, and the highest one thousand yards. Under favourable circumstances the former will attain about seventy yards, and the latter between eight and nine hundred yards of altitude; and the intervening three kites will float at different altitudes between these two extreme ones of the series. If now, we place the ball of a Leyden jar to the insulated string of the lowest kite, it will become charged to a low intensity in a certain period of time; and by applying the ball of the jar to an electroscope, and testing the character of the electric action, either by sealing wax or by glass, in the usual way, we find that it is positive.

The same operations are to be proceeded with at the other kite strings, and it will be found that the whole of them display positive electric action; and with an increase of intensity in the charge of the jar, from the first to the highermost kite in the series. We next proceed to ascertain the *relative difference* of the electric actions of the kite strings, by bringing two of them at a time in their insulated state, close to each other, and we observe a spark pass between them; and after satisfying ourselves by these means that, although the whole series are positive to the ground, they are positive and negative with respect to each other: we next insulate a jar, and by connecting one kite string with its exterior surface, and another with its interior one, the jar becomes charged, and in such a manner, that the highest or longest string of a pair, invariably communicates the positive charge to the glass, whether it be the inside or outside of it with which it is in contact.

There are, however, frequent cases, whilst experimenting with kites, in which that with the longest string will not be the highest; under such circumstances the *intensity* of the charge of such a string, is not so great as that of a shorter, whose upper end is much higher; although the *quantity* obtained by a discharge from the former is occasionally somewhat greater than that discharged by the latter. Now, since there is a general law in electricity by which we are enabled to understand that the fluid cannot be transmitted from one body to another, unless the former be positive to the latter, it becomes obvious that the exploring wires in the kite string

must have been negative electrical to the atmosphere, prior to their receiving their respective charges from it; and as those electric charges were not communicated to the wires prior to their ascent, it is obvious, also, that they were received from those portions of air through which they ascended, and eventually from those regions which were invaded by their highest points. And since the electric charges are found to be the highest from the highest altitudes the electric pressure is also greater in those places: hence the exploring wires become electro-polar, having their lower and upper parts, *positive* and *negative* respectively. And since the circumambient pressure at the lower ends of the wires is uniform, the electric state which each wire displays will depend on the electric state of the air surrounding its highest point; and consequently, the relative electric actions exhibited by the lower ends of the kite strings are true representatives of the relative electric conditions of those strata of air in which the kites were floating.

The same fact may be proved by the opposite process. If, for instance, from a balloon at a great altitude, several insulated wires of different lengths, were to be let down by weighting the lower extremities, each wire would be found negative electrical with respect to the balloon itself, and consequently with respect to its contents and the surrounding air. The shortest wire, in this case, would be the *least electrical,* and the longest one the *most electrical* in the series; and every one in the series would be negative electrical on some function of its length, no two of them being alike. Hence, if the longest wire touched the ground, it would be negative to the whole of the remaining part of the series. By this process, as decidedly as by that with the kites, the wires would become electro-polar; and because the lower extremities of all the wires would be subjected to a *less* degree of electric pressure than that investing their upper extremities; the lower pole of each wire would be electro-positive. The polar action, however, would be of different degrees of intensity, and consequently, the lower ends of the wires would relatively display different electric states; being positive and negative throughout the series. The lowermost wire (if insulated from the ground) being *positive,* and the uppermost one *negative* to all the rest. Hence we understand, that it is a matter of no consequence whether the wires proceed from one common station *downwards,* or from one common station *upwards,* they will, in either case, be *relatively* positive and negative accordingly with their respective lengths, when in a vertical position.

Again, since the different *degrees* of polarity, or different electric states of the extremities of the wires, depend upon different degrees of electric pressure at the upper and lower stations, it is obvious that if the pressure was equal from one end of the wire to the other, no electro-polarity could possibly take place. Moreover, since no electric discharge can possibly happen independently of polarity, we learn the reason why balloons have passed with impunity through dense clouds highly charged with the electric matter (thunder

clouds), not experiencing even the slightest indication of electric action; excepting, perhaps, the unpleasant sensation which the aeronaut might experience from the great circumambient electric pressure.

It is to the same cause that the want of actiou was discernable by the *Scrutator* in the interior of Beccaria's *electrical well*, and by Dr. Faraday whilst living in an *electrical cage*. Both were surrounded by equable electric pressures, and consequently where no electric phenomena could happen.

I have already informed you, that the quantity of electric matter in bodies generally, is almost continually varying, from the ever-varying surrounding electric pressure; and that different bodies have different degrees of susceptibility for the reception of the electric fluid. Hence it will be easy to conceive that any insulated body of *great* dimensions, would receive a greater quantity of the electric fluid than a small one of the same kind, when both were under the same degree of pressure. Hence it is, that a long insulated copper wire stretched *horizontally* at the height of a few yards from the ground, will, occasionally, even when no cloud is present, receive a considerable charge, sufficient, indeed, to communicate powerful shocks, from the surrounding air; although a very *short* piece of the same wire, would never receive an appreciable charge under any circumstance of surrounding pressure in a cloudless sky. But it must be observed, that such a wire being surrounded by an equable electric pressure, cannot possibly become electro-polar, and consequently, could not dispose of any of its charge to a vicinal insulated wire of the same kind, whether the lengths were equal or unequal; unless, indeed, the latter wire had not been exposed to the same pressure for a sufficient period of time, to receive all the fluid due to its susceptibility of being charged with from that pressure.

Now, although no wire could dispose of any part of such a natu ral charge of the electric fluid to another of the same kind and similarly situated, the distribution of its fluid would suffer a change by the approach of the other wire in certain manners. If, for instance, the two wires approached one another longitudinally, until they came close together, and laid side by side from one end to the other. Under these circumstances, the fluid previously occupying those parts of the two surfaces, which, after contact, forms the *plane* of contact of the wires, would become displaced by mutual repulsion; and after a moment's disturbance of the whole of the fluid in both wires, a new distribution and electro-equilibrium would be established.

With respect to the approach of insulated matter of other kinds, to that constituting the insulated wire, it is obvious that electrical pehenomena would be displayed in consequence of a difference of electric pressures which the bodies would exercise on each other as the surfaces approach. If the approaching body were uninsulated, a polarization would take place, whatever were the character of that body; because the electric pressure on the wire would be lessened

at, and about the point approached: it would there exhibit a *positive* pole; and if the body approached near enough, a discharge from the wire would take place, the force of which would be proportional to the extent of the wire and the electric pressure of the surrounding aerial medium at that time; and as that pressure is continually varying, the charge in the wire will vary also. Therefore the same wire cannot be charged to the same extent at all times, which is a fact well ascertained by several electricians of the highest repute. From the whole of these circumstances, a long insulated wire appears to be well adapted for an important part of an electro-phorascope, which would indicate the changes of electric pressure in the lower strata of the atmosphere, under a cloudless sky; and as such a wire would always represent the electrical state of that stratum of air in which it was suspended, it would also form an important part of an *electro-metaboliascope,* which would indicate those changes from *plus* to *minus,* and *vice versa,* of the air, with respect to the earth, which occasionally take place by the approach, transit, and departure of clouds.

On the Induction from the Discharge of the Battery. *By* M. Ch. Matteucci.*

When I undertook the study of the secondary current developed by induction, by means of a current from the Leyden jar, I took as a measure of the direction and of intensity of this current, the degree and direction of the magnetism which it developes in a steel needle which is contained in a cylindrical spiral *dextrorsum* traversed by this current of induction. M. Henry and M. Marianini, who were occupied at the same time in the study of this phenomenon, have employed the same means; M. Marianini alone made use of temporary magnetism. Without knowing the works of each other, we have published our results, and they are found to agree perfectly. These results may be reduced to the following:—Firstly: If the discharge is feeble, the secondary current is directed in an inverse direction to the current of the jar: in all other cases it has the same direction as the latter. Secondly: If the two currents are removed from each other, we always arrive at a certain distance at which the secondary current is found to be reversed. Thirdly: Any intermediate metallic plates destroy the secondary current by beginning to produce it in an inverse direction, and at the same time the secondary current travelling in the same direction as that of the jar, traverses the plate. M. Reiss has also studied this same subject, making use of a thermoscope to measure the intensity of the secondary current. He obtained some remarkable results, which may in some measure be resolved into those which we obtained. This philosopher accuses

* *Archives De L'Ectricité.* No. 1.

the process which we employed to determine the direction of the current, of uncertainty, in referring to the beautiful work of M. Savary, which proves how the direction and the degree of magnetization are variable in circumstances which have not yet been subjected to any general law.

Since my first researches I have reflected a good deal on the results of M. Savary; and it will be seen in my memoir, that by employing very feeble discharges and very long circuits, I have endeavoured to place myself in conditions in which the magnetic action of the current no longer presents anomalies. I persist in believing that the indication of magnetism is always correct for measuring the intensity and the direction of the current. I hope that ere long it will not fail to be proved. I believe this process to be more sensitive than all the others which we have employed, and capable of indicating to us, in the duration and force of the current, many variations which escape other methods. I have at all times been desirous of trying if the galvanometer might be employed in these researches; I have taken for this, Gourjon's galvanometer, such as Melloni employed in his researches on heat. I have always employed my two spiral planes, each of which is composed of twenty-three metres of copper wire. These two spirals are brought very near to each other, and are only separated by a plate of very thin glass. It is with this apparatus that I have obtained for the first time very brilliant sparks of induction, by employing one of the smallest Leyden jars that can be made with a smelling bottle. I soon perceived that, in order to have signs from the galvanometer, it would be necessary to employ a battery. The batteries that I have employed consisted of 4, 8, 10, or 12 jars, and had 0 m.c., 3864, 0 m.c., 6828, 0 m.c., 9660, 2 m.c., 152, of coated surface.

Before detailing the results which I obtained, I confess that, particularly with charges of a very slight degree of tension, I have always seen a spark escape in the circuit of the galvanometer. It would have been necessary to employ a galvanometer the wire of which was well covered with varnish, like those of Colladen. I obtained very constant numbers, but I still refrain from giving them as invariable, and as establishing a law numerically expressed. The spark which is given out in the circuit of the galvanometer ought always to destroy a portion of the action of the current, and this portion ought to be variable in each case. With a battery of twelve jars I have obtained the following numbers:—

Tension of the Battery.	Deflection.
10°	4°
20	8
30	12
40	16

The direction of the secondary current is always the same as that of the current of the jar, which goes from the surface which communi-

cates with the conductor of the machine, to the other which communicates with the ground. Moreover, if we charge this battery up to the point where we perceive the noise of the spontaneous discharge, the needle of the galvanometer is pushed on to 90°. I have compared the effects of two batteries of four and of eight bottles, for the same degree of tension, and I obtained for the secondary currents the following numbers :—

Tension.	With 4 bottles.	With 8 bottles.
10°	3°	4°
20	4 ex.	7 ex.
30	6	9
40	8	10
50	10	16

The direction being still the same as in the preceding case. We see clearly that the intensity of the secondary current increases with the tension and with the quantity of electricity which is discharged through the primitive circuit. If we make the distance between the two spirals to vary, the secondary current does not change its direction; it becomes enfeebled with rapidity, and is reduced to zero; but we never obtain an inverse current, as is indicated by the process of magnetizing employed with very feeble discharges. Here are some of the numbers; the charge was from eight jars, and constantly at 40° of tension :—

Distance between the two spirals.	Deflection of the galvanometer.
0^{m}·, 003	17°
0 , 01	9 ex.
0 , 02	8
0 , 06	4
0 , 12	Zero.

In order to establish the law which ought to connect the direction of this secondary current of the Leyden jar with the effects of voltaic induction discovered by Faraday, I have attempted to multiply the number of spirals, in order to oblige the secondary current to become primitive in another circuit. The experiment succeeded very easily. I constructed three pairs of spiral planes of the same copper wire, and of the same length as the two preceding spirals. This is a very remarkable experiment of uniting the functions of these three pairs of spirals in such a manner as to obtain the spark. The disposition of the experiment is very simple: we add to the second spiral—that is to say, to that in which the primary induction takes place—another spiral, and at a certain distance. A fourth spiral is placed like the second and the first, in face of the third. To this fourth spiral we again add a fifth, and in face of this latter we put as for the second and third, a sixth spiral. We dispose the circuit in such a manner that there is an interruption in the circuit of the second and third, another in that of the fourth and fifth, and then a latter one in that of the sixth. It is sufficient if we cause to pass the discharge from

a very small jar through the first circuit, to make instantly visible at all points of interruption a spark which seems everywhere to have the same strength and lustre. My first researches have been directed with the end of establishing the direction of the different currents, induced and inducing at the same time. I commenced with two pairs of spirals, and employing two galvanometers in communication with the second and with the fourth spiral. My experiments have been made with the battery of twelve jars, from 10° up to 40° of tension. The pairs of spirals were only separated by a plate of very thin glass. The following table presents the results of my experiments, which I have repeated several times with M. Pacinotti, my colleague, who has considerably aided me in these researches. (Fig. 1). When we employ three pairs of spirals in place of two, the results with the same discharges are produced in the same direction. (Fig. 2). We see very easily that when the induced current becomes an inductor, the current which it develops is always directed as it would be if the induction was produced by a voltaic current which originates it; that is to say, that the direction is the inverse of that of the inducing current. This is precisely the contrary of that which happens to the first current of induction which the discharge produces: the current of induction has the same direction as the inducing current; it acts, consequently, as a voltaic current which intermits.

I wished to study the relations of intensity of these different currents of induction. I always give the numerical results, still being a little diffident of the correctness. At first the needles of the galvanometer lose much of their degree of magnetism by the effect of these currents, and there is always some little spark which explodes. It is for this reason that in all these experiments I have never taken notice of any result without ascertaining whether, in reversing the position of the extremities of the galvanometer, the direction of the deflection was equally reversed. By employing the same galvanometer successively united to the spirals 2, 4, 6, and with the same charge of 10°, I obtained 7° ex., 3°, 1°. These numbers give the intensity for the current of induction of the first, second, and third order.

It was important to study the direction of the current when the spark was given out in the secondary circuit. Having made some trial with the galvanometer, I found that the deviation was hardly sensible in this case, and that even when discharging my jars directly, the deviation was not so great as with my currents of induction. I employed, in place of the galvanometer, a very simple process which was suggested to me by M. Pacinotti. It is that of the hole which the electric spark always makes in paper near to the negative point. The apparatus is very simple, very sensitive, and constant in its results. By interrupting my circuit of the second and third order, I had sparks of induction which yet made a very visible perforation. I took two plates of pewter terminating in a point. I pasted them, one above and the other underneath a morsel of common paper, leaving the two points at two millimetres distance between each other

When the spark was given out a hole was found at the negative extremity. Ordinarily we obtain another sign equally constant. It is that of a black stain near the positive point. In a very great number of experiments that I have made, the phenomenon of the hole at the negative point has always been constant; the other sign very rarely fails. I then disposed several of these pieces of apparatus in my spirals; that is to say, at the second, at the fourth, and at the sixth spiral. We see in every place, with a discharge of 10° of the battery of twelve jars, that the hole is formed, and the direction of the different currents of induction may be seen very readily in the following tables. (Figs. 3 & 4). The law of the direction of the currents of induction produced by the discharge of the battery, and of currents of induction which the induced currents develope, is evident. When the secondary circuit is open, and there is a spark, the secondary current is directed in an inverse direction to the primary current, and this result is verified for the same current of the jar. In fact, the first current of induction has a direction opposite to that which it has when the circuit is closed. The induction is then subject to the same law as voltaic currents at the commencement of their action. We can see what happens when we compare closed circuits with those which are broken. We find everywhere verified that which we saw occur to the first secondary current: if the circuit is closed, the secondary current has the same direction as the current of the jar. I here give several tables which evidently prove it, and the results of which have been many times confirmed. (Figs. 5 and 6). We see, then, that whichever of the secondary currents it is that we take, the current which is developed by induction is always in the same direction as the inducing current, if its circuit is closed, the other being open; or if its circuit be open, the other being closed. If we hold the galvanometer in the open circuit, we obtain very small deflections, and hardly sensible to the instrument I employed. I have always found that when the two ends were very near each other, the needle deviated, giving the same indication as the hole. If we remove the points more remote from each other, the indication of the galvanometer will appear reversed. This is a subject which merits a profound study, and for which I have already disposed apparatus with M. Pacinotti. Perhaps the galvanometer falls short, in this case, by the inductive actions which are always contrary to the current which is commencing, and similar to the current which is ceasing, not being separated. This is a question which we may perhaps resolve when we can obtain signs of induction by a discharge from a battery slowly produced, in such a manner as to see, when in darkness, a continued current of light. Hitherto all the attempts which have been made to accomplish this have been useless. I am persuaded that it is necessary further to insulate the wire of the galvanometer and that of the spirals.

I have only made a small number of experiments on the action of interposed plates between the spirals. At first I said that if we oblige the primitive current of the battery to produce two secondary

currents, by placing it in the middle of two spirals, the current of induction which it developes in each of these spirals is always directed as when there is only a single secondary spiral, and the intensity of the current is equal to that which it developes when we suppress one of these secondary spirals. One of these secondary spirals has not any effect on the enfeebling of the secondary current, if its circuit is not closed. On the contrary, if we put in contact with the secondary spiral another spiral closed, in such a manner as to place it between the spiral which is traversed by the discharge from the battery and another precisely similar, the secondary current has the same direction as it would have without this spiral, and its intensity appears augmented.

The action of non-conducting plates interposed is nothing. If we employ metallic plates, the secondary current is considerably enfeebled, but its direction is not changed. It is thus that a plate of zinc of 1½ millimeters of thickness destroys the current of induction from the discharge of eight jars at 40°, and at the distance of $0^m, 01$, between the two spirals. A very thin plate of pewter has no influence on this discharge; it requires five, one on the other, to reduce the deviation to 5°, from 9°, when it was without plates. A plate of silver and copper, very thin, hindered the production of the secondary current. It appears, then, that in order to reduce from a given quantity the property which the current possesses of producing induction, it is necessary to interpose a metallic plate whose thickness shall be in inverse ratio to its conductibility.

I was desirous of trying if it was possible to obtain any signs from the galvanometer, by cutting the plate of pewter, and making the two points of the cut disc touch the two wires of the galvanometer. I could obtain nothing with the strongest discharges. This is sufficient to prove the great superiority of the process of magnetism. I prepared a disc of pewter whose diameter was four times greater than that of my spirals. By soldering the wires united with the cylindrical spirals in different parts of this disc, I obtained, with very feeble discharges, a strong magnetization on the steel needles which were introduced into my spirals.

The process of magnetizing merits a more profound study. Hitherto I have not been able to obtain any signs of action in the galvanometer with the discharge from a small bottle which constantly gives a secondary current capable of magnetizing, as a voltaic current would do, directed in a contrary direction to the current from the jar or bottle. This fact is constant.

Pisé au Cabinet d' Phys. del Université,

10 Fevrier, 1841.

[NOTE.—The tables and figures referred to, are not in the original; and therefore some mistake must have occurred in the printing of it.—EDIT.]

On some peculiar Phenomena exhibited in a Solution of Nitrate of Silver, traversed by an Electric Current. By M. C. Matteucci.*

In a former memoir I have described the following fact. When an electric current is transmitted through a solution of nitrate of silver, a black flocculent matter is deposited on the negative platinum terminal; but which becomes instantaneously transformed into a silvery appearance whenever it falls from the terminal into the liquid, or when the current ceases to be transmitted.

The production of these phenomena can be accomplished only under certain circumstances. If the solution be strong, the silver is always liberated in a very well crystallised state: and it is only by employing a very dilute solution that we succeed in producing the above described phenomena. If the solution be of 100 water to 1 of nitrate of silver, the black flocculent matter is readily produced: but when the water is in greater proportion, it no longer appears. Its production also depends upon the power of the current, which must be stronger as the solution is less charged with the salt.

Whether the silver be liberated in the flocculent or in the crystalline state, the quantity does not vary to a sensible extent; being equal to that obtained by calculation founded on the quantity of water decomposed in another part of the same electric current. When a portion of the transformed silvery matter is placed between the platinum terminals in a solution of nitrate of silver, those parts of it near to the positive terminal become blackened, but suffer no change by the current when similarly situated in liquids containing no nitrate of silver; though a single drop of the nitrate in them would cause the blackness to appear.

The black matter is transformed to the silvery white state whenever the platinum terminal to which it is attached is shaken in the liquid; but returns to its blackness by letting fall upon it a stream of a similar nitrate solution. If, whilst still attached to the negative platinum on which it was formed, the black flocculent matter be transferred to a *strong* solution of nitrate of silver, it suffers no change by the new current, but becomes covered with crystallised silver, which forms upon it in the same manner as it would appear on the platinum terminal alone.

The formation of the black matter occurs whether the terminals in the solution be four inches or only one inch apart. If a slip of glass be placed between them the deposit will be white: but becomes black when the glass is removed. This fact, I think, is attributable to the different degrees of intensity which the current assumes by the presence or absence of the glass; for, as the deposited matter which forms is black, with great distances between the terminals, the absence of all colour when the glass interposes, cannot be attri-

* *Les Archives de L'Electricité.*

buted to any action which the polar products exercise on one another; which, under such circumstances, could not happen.

Acetate of silver, when dissolved in a large proportion of water, affords the same phenomena as the nitrates: and they as uniformly appear when to the solution an excess of the nitric and sulphuric acid has been added. We might suppose that the black deposited matter is composed of oxide of silver which is protected by the transmission of the current; but which, when the current ceases to flow, immediately assumes the metallic state. This, however, is an hypothesis which must be considered as perfectly gratuitous, for I must confess that, hitherto, I am unable to account satisfactorily for the phenomena.

Pisa, July, 1871.

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*Historical Sketch of Professor Schoenbein's Experiments on a peculiar Voltaic Condition of Iron.**

The first series of these experiments on record that we are acquainted with, are described in a letter to Dr. Faraday, dated Bâle, May 17, 1836. They are as follow :—

"If one of the ends of an iron wire be made red hot, and after cooling be immersed in nitric acid, sp. gr. 1·35, neither the end in question nor any other part of the wire will be affected, whilst the acid of the said strength is well known to act rather violently upon common iron. To see how far the influence of the oxidized end of the wire goes, I took an iron wire of 50′ in length and 0‴·5 in thickness, heated one of its ends about 3″ in length, immersed it in the acid of the strength above mentioned, and afterwards put the other end into the same fluid. No action of the acid upon the iron took place. From a similar experiment made upon a cylindrical iron bar of 16′ in length and 4‴ diameter, the same result was obtained. The limits of this protecting influence of oxide of iron with regard to quantities I have not yet ascertained; but as to the influence of heat, I found that above the temperature of about 75° the acid acts in the common way upon iron, and in the same manner also, at common temperatures, when the said acid contains water beyond a certain quantity, for instance, 1, 10, 100, and even 1000 times its volume. By immersing an iron wire in nitric acid of sp. gr. 1·5 it becomes likewise indifferent to the same acid of 1·35.

"But by far the most curious fact observed by me is, that any number of iron wires may be made indifferent to nitric acid by the following means. An iron wire with one of its ends oxidized is made to touch another common iron wire; both are then introduced into nitric acid of sp. gr. 1·35, so as to immerse the oxidized end of the one wire first into the fluid, and to have part of both wires above the level of the acid. Under these circumstances no chemical action upon the wires will take place, for the second wire is, of course, but

* *Phil. Mag.*

a continuation of that provided with an oxidized end. But no action occurs, even after the wires have been separated from each other. If the second wire, having become indifferent, be now taken out of the acid, and made to touch at any of its parts, not having been immersed, a third wire, and both again introduced into the acid so as to make that part of the second wire which had previously been in the fluid enter first, either of the wires will be acted upon either during their contact or after their separation. In this manner the third wire can make indifferent or passive a fourth one, and so on.

"Another fact, which has as yet, as far as I know, not been observed, is the following one. A wire made indifferent by any of the means before mentioned is immersed in nitric acid of sp. gr. 1·35, so as to have a considerable part of it remaining out of the fluid; another common wire is put into the same acid, likewise having one of its ends rising above the level of the fluid. The part immersed of this wire will, of course, be acted upon in a lively manner. If the ends of the wires which are out of the acid be now made to touch one another, the indifferent wire will instantly be turned into an active one, whatever may be the lengths of the parts of the wires not immersed. (If there is any instance of chemical affinity being transmitted in the form of a current by means of conducting bodies, I think the fact just stated may be considered as such.) It is a matter of course, that direct contact between the two wires in question is not an indispensably necessary condition for communicating chemical activity from the active wire to the passive one; for any metal connecting the two ends of the wires renders the same service.

"Before passing to another subject, I must mention a fact, which seems to be one of some importance, An iron wire curved into a fork is made to touch at its bend a wire provided with an oxidized end; in this state of contact both are introduced into nitric acid of sp. gr. 1·35 and 30°, so as first to immerse in the acid the oxidized end; the fork will, of course, not be affected. If now a common iron wire be put into the acid, and one of the ends of the fork touched by it, this end will immediately be acted upon, whilst the other end remains passive; but as soon as the iron wire with the oxidized end is put out of contact with the bend of the fork, its second end is also turned active. If the parts of the fork rising above the level of the acid be touched by an iron wire, part of which is immersed and active in the acid, no communication of chemical activity will take place, and both ends of the fork remain passive; but by the removal of the iron wire (with the oxidized end) from the bend of the fork, this will be thrown into chemical action.

"As all the phenomena spoken of in the preceding lines are, no doubt, in some way or other dependent upon a peculiar electrical state of the wires, I was very curious to see in what manner iron would be acted upon by nitric acid when used as an electrode. For this purpose I made use of that form of the pile called *couronne des tasses*, consisting of fifteen pairs of zinc and copper. A platina wire

was connected with (what we call) the negative pole of the pile: an iron wire with the positive one. The free end of the platina wire was first plunged into nitric acid sp. gr. 1·35, and by the free end of the iron wire the circuit closed. Under these circumstances the iron was not in the least affected by the acid; and it remained indifferent to the fluid not only as long as the current was passing through it, but even after it had ceased to perform the function of the positive electrode. The iron wire proved, in fact, to be possessed of all the properties of what we have called a passive one. If such a wire is made to touch the negative electrode, it instantaneously becomes an active one, and a nitrate of iron is formed, whether it be separate from the positive pole or still connected with it, and the acid be strong or weak.

"But another phenomenon is dependent upon the passive state of the iron, which phenomenon is in direct to contradiction all the assertions hitherto made by philosophical experimenters. The oxygen at the anode arising from the decomposition of water contained in the acid, does not combine with the iron serving as the electrode, but is evolved at it, just in the same manner as if it were platina, and to such a volume as to bear the ratio of 1 : 2 to the quantity of hydrogen evolved at the cathode. To obtain this result I made use of an acid containing twenty times its volume of water; I found, however, that an acid containing 400 times its volume of water still shows the phenomenon in a very obvious manner. But I must repeat it, the indispensable condition for causing the evolution of the oxygen at the iron wire is to close the circuit exactly in the same manner as above mentioned. For if, *exempli gratia*, the circuit be closed with the negative platina wire, not one single bubble of oxygen gas makes its appearance at the positive iron; neither is oxygen given out at it when the circuit is closed, by plunging first one end of the iron wire into the nitric acid, and by afterwards putting its other end in connexion with the positive pole of the pile. In both cases a nitrate of iron is formed, even in an acid containing 400 times its volume of water; which salt may be easily observed descending from the iron wire in the shape of brownish-yellow coloured streaks.

"I have still to state the remarkable fact, that if the evolution of oxygen at the anode be ever so rapidly going on, and the iron wire made to touch the negative electrode within the acid, the disengagement of oxygen is discontinued, not only during the time of contact of the wires, but after the electrodes have been separated from each other. A few moments holding the iron wire out of the acid is, however, sufficient to recommunicate to it the property of letting oxygen gas evolve at its surface. By the same method the wire acquires its evolving power again, whatever may have been the cause of its loss. The evolution of oxygen also takes place in dilute sulphuric and phosphoric acids, provided, however, the circuit be closed in the manner above described. It is worthy of remark that the disengagement of oxygen at the iron in the last-named acid is much easier stopped,

and much more difficult to be caused again, than is the case in nitric acid. In an aqueous solution of caustic potash, oxygen is evolved at the positive iron, in whatever manner the circuit may be closed, but no such disengagement takes place in aqueous solutions of hydracids, chlorides, bromides, iodides, fluorides. The oxygen, resulting in these cases from the decomposition of water, and the anion (chlorine, bromine, &c.) of the other electrolyte decomposed combine at the same time with the iron.

"To generalize these facts, it may be be said, that independently of the manner of closing the circuit, oxygen is always disengaged at the positive iron, provided the aqueous fluid in which it is immersed do not (in a sensible manner) chemically act upon it; and that no evolution of oxygen at the anode in contact with iron under any circumstances takes place, if besides oxygen another anion is set free possessed of a strong affinity for iron. This metal having once had oxygen evolved at itself, proves always to be indifferent to nitric acid of a certain strength, whatever may be the chemical nature of the fluid in which the phenomenon has taken place.

"I have made a series of experiments upon the silver, copper, tin, lead, cadmium, bismuth, zinc, mercury, but none showed any resemblance to iron, for all of them were oxidized when serving as positive electrodes. Having at this present moment neither cobalt nor nickel at my command, I could not try these magnetic metals, which I strongly suspect to act in the same manner as iron does.

"It appears from what I have just stated that the anomalous bearing of the iron has nothing to do with its degree of affinity for oxygen, but must be founded upon something else. Your sagacity, which has already penetrated into so many mysteries of nature, will easily put away the veil which as yet covers the phenomenon stated in my letter, in case you should think it worth while to make it the object of your researches.

"Before I finish I must beg of you the favour of overlooking with indulgence the many faults I have, no doubt, committed in my letter. Formerly I was tolerably well acquainted with your native tongue; but now, having been out of practice in writing or speaking it, it is rather hard work to me to express myself in English.

"It is hardly necessary to say that you may privately or publicly make any use of the contents of this letter.

"I am, Sir, your most obedient Servant,

"C. T. Schoenbein,

"Prof. of Chem. in the University of Bâle."

"Bâle, May 17, 1836."

Further Observations on the Action of Nitric Acid upon Iron. By Dr. Schoenbein.

I have already remarked that nitric acid, which generally attacks iron with violence, has no action upon an iron wire, one end of which, before its immersion in the acid, has been heated to dullness. From experiments since made, I find that the action is dependent on the quantity of water combined with the acid. This quantity I have not yet accurately ascertained, but I find that acid of the sp. gr. of 1·36 diluted with 15, 30, 60, 120, 240, 480, and 960 times its volume of water, attacks an iron wire heated at one end in the same manner as when not heated, and that the oxidated iron falls off by degrees into the acid without being dissolved in it.

Diluted nitric acid acts upon iron wire protected by platina or gold in the same manner as when the end of the wire is heated. Many chemists state that iron is not acted upon by ordinary nitric acid when diluted with three times its volume of water: according to my experiments, this metal is sensibly dissolved by nitric acid diluted with 1000 times its volume of water. As it is evident that the different action of the same nitric acid on iron is caused by a certain electrical state of the metal, I endeavoured to ascertain its nature by making an iron wire the positive pole of a voltaic battery set in action by nitric acid. I experimented in the following manner:—

Nitric acid, of sp. gr. 1·36, at the ordinary temperature, was used in a circle of fifteen plates, with a voltaic cup apparatus; at the positive pole an iron wire, and at the negative a platina wire, dipped into the acid. When I closed the circuit with the negative wire, the iron wire was acted upon as usual; when I closed it with the iron wire, by first dropping one end in the nitric acid, and then making the other end the positive pole of the battery, the same effect also took place; but when I closed the circuit so that one end of the iron wire was first united with the positive pole, and the other end afterwards dipped into the acid, no action took place on the iron, and it possessed, after its separation from the positive pole, all the properties which it had by heating, or when protected by gold and platina,—precisely those which I have already so fully stated in my former paper. I heated the nitric acid used in the circuit to nearly its boiling point before it acted upon the positive iron wire. It follows, of course, that under these circumstances the water contained in the nitric acid was decomposed. No hydrogen gas is given out at the negative pole from strong nitric acid, for instance, of sp. gr. 1·36, but it combines with a part of the oxygen of the acid, and converts the latter into nitrous acid.

At a temperature of 70° centigrade, a gas is given out at the negative platina wire, which I have not yet particularly examined, but which is probably deutoxide of nitrogen. It has hitherto been

considered that the other element of the water, the oxygen, combines with the positive iron wire, and forms a hydrate with the nitric acid.

If the circuit is so formed that the nitric acid has no action upon the iron wire, the free oxygen does not combine with the metal, but is given off in a gaseous state, precisely as when silver, gold, or platina wires are used. This is not only the case with acid of the above strength, but also with acid diluted with 1, 10, 100, and even 400 times its volume of water. That the iron is not partially oxidated is evident from its unchanged metallic lustre, as also from the proportions of gas given off at both wires, which I found according to several measurements to be as 1 to 2. If the two wires where the water is being decomposed are brought for a few seconds into contact (the nitric acid being diluted with about ten parts of water), and then again separated, oxygen gas is no longer given out at the positive iron wire, but a yellow nitrate is formed, which sinks to the bottom. If, however, the end of the iron wire which has been dipped into the nitric acid is exposed to the air for a moment, and the circuit then closed, oxygen gas is again given off from the iron. If the communication is broken by means of the negative wire, and again made, after a few seconds oxygen continues to be given out at the positive iron wire. When the acid is very much diluted, it requires some time after closing the circuit before the oxygen gas appears. In whatever manner the giving out of the oxygen gas is interrupted, it always recommences if the iron wire is exposed for a short time to the air, and the circuit then formed by it in the usual way. Exactly the same effects take place when diluted sulphuric or phosphoric acid is substituted for nitric acid.

In order that oxygen gas should be given off by these acids at the positive iron wire, it is first necessary that the negative pole should be in communication, by means of a wire, with the decomposing fluid; that one end of the iron wire should be in contact with the positive pole, and the other end with the acid. By any other mode of closing the circuit, oxygen gas is not given out, even when the iron wire has been previously dipped in diluted sulphuric or phosphoric acid. The giving out of oxygen gas will be interrupted upon bringing the wires of the poles into contact when the last-mentioned acids are used, and will not return upon exposure of the iron wire to the air. No oxygen gas is given out at the positive iron wire when subphosphorous or phosphorous acid dissolved in water is substituted, probably because it combines with the acid: when diluted alkalies are used, oxygen gas is given out at the positive end, in whatever way the circuit may be closed. During my experiments I have observed many other singular phenomena, which I shall communicate hereafter, when the circumstances under which they occurred are better understood. One of these, however, I will mention in conclusion: in the same nitric acid in which a platina wire served for the negative pole, a quantity of hydrogen gas was given off, but when an iron wire was substituted for the platina, no gas

appeared. It was only when a considerable time had elapsed after the substitution that hydrogen made its appearance.

From several of the above-mentioned facts, it appears that the law formerly laid down, that oxygen gas is only given out at the positive pole of a battery when it is terminated by a noble metal, does not hold good; that the same cause which renders an iron wire indifferent to nitric acid, prevents the oxidation of this metal during the decomposition of water by means of the galvanic battery. I shall not for the present enter further into the theory to be deduced from these facts, as I am satisfied many further researches still require to be made.

New Researches on the Properties of Electric Currents, discontinued and directed alternately in contrary directions. By Professor M. A. De la Rive.*

It is now three years since I published, in the *Memoires de la Société de Physique et d'Historie Naturelle de Geneve*, some *researches on the properties of magneto-electric currents.* I undertook, in that memoir, the task of making a more profound study of certain points, and of analyzing, better than I had been able to do in the former essay, the cause of some of the phenomena which I have there described. I have not lost sight of this engagement, and I feel myself more called on to fulfil it, as my preceding work has been the object of some critical observations on the part of a German philosopher, M. Lenz, to which observations I feel desirous of responding. The title alone which I give to the memoir I now publish is already a response; for one of the most important objections which M. Lenz has made to me is, as will be seen, that of having designated the currents of which I have made use, under the name of *magneto-electrical currents*, and of having thus attributed, to the fact, that they were produced by magnets, an influence quite peculiar and almost mysterious. Now, as I shall hereafter explain, I have not attached any other importance to the denomination *magneto-electric*, than that of indicating clearly what was the means by which I had procured the currents which formed the object of my study. I know very well the circumstance which characterised and distinguished them from other currents, and this is the fact of being discontinued and directed alternately in contrary directions. It is this double character which appeared to me to

* The memoir which I now publish will appear at the same time in the first part of tome ix, of the *Memoires de la Société de Physique et d'Historie Naturelle de Geneve*, under the title of *some chemical phenomena which manifest themselves under the action of electric currents devetoped by induction.* I have solely made some additions and some changes of form. I have in it also retrenched all one part, which I intend to print in the next number of the *Archives de l'Ectricite*, with more considerable development than those which I have given in the memoir to which I have just referred.

give to this species of currents an importance of which experience has to me confirmed the reality, for it has shown me that the employment of them might conduce to the discovery of phenomena of an entirely new order, and of a true interest to the scientific world.

Currents discontinued and directed alternately in contrary directions may be developed by many different methods, and not only by magnets. It becomes convenient, then, to designate them by that which constitutes their principal character, either from their essence, so to speak, or rather by some accessary circumstance, such as one of the manners of producing them. This is what I am about to do at present; and I do it much more willingly, as my aim is precisely to endeavour, still more than in my preceding work, to extract from among my experiments, some few experiments which make these currents differ from the ordinary currents of the voltaic pile, with which I shall be called to compare them.

Before entering upon the exposition of the new experimental researches which I have made, I shall examine the critical observations of M. Lenz, and endeavour to respond to them. This examination, and these responses, which have naturally led me to take up again some points of my former work, will serve also as an introduction to that part of this second memoir, which contains the new researches.

PART FIRST.

An examination of the Critical Observations of M. Lenz, *on my Memoir, entitled* Researches on the Properties of Magneto-Electrical Currents.*

M. Lenz has published in the *Annalen der Physik*, of Poggendorf,† some observations on my preceding memoir above referred to; he has classed them under the same heads as my researches are classed. I shall follow exactly the same order in the examination which I am now about to make.

1. *General Observations on Magneto-Electric Currents.*—M. Lenz commences by combatting the opinion that it is possible to have a specific difference between electric currents of different origin; he does not admit that, amongst these currents, that some of them are more particularly adapted to the production of chemical effects, and others for the production of calorific effects; he rejects the idea that the relation, as to the conductibility of bodies, is not the same for powerful currents as for those of a feebler character. He attributes the errors into which, according to his account, philosophers have fallen, in this respect, to the ignorance prevailing among them of the law of Ohm; a law, the discovery of which appeared to him to be one of the most important which has been made in voltaic electricity.

* *Archives De L'Ectricité*, No. 1.
† *Ann. der Physik*, vol. xlviii, p. 385.

Without denying the importance of the law of Ohm, I may be permitted to remark, that it is necessary to distinguish the circumstances which this law has for its object, to appreciate its influence, in the form under which he takes it into account. As to these circumstances, I have enumerated them myself, in a memoir published in the *Annals of Chemistry*, of March, 1828;* I have expressly indicated that the intensity of an electric current in a voltaic pair, depends not only on the relative energy of the two metals for the producing cause to ascertain the chemical action, according to my theory, but that it depends also on the resistances which the current meets with in its passage through the circuit. I have indicated that these resistances are the imperfect conductibility of the solids, and more especially the liquids which compose the circuit, and the difficulty which electricity has to encounter in alternately traversing liquid and solid conductors. M. Ohm admits that the intensity of the current is a function of two of the three causes of resistance which I have just referred to; but he has done more than I have: he has determined the form of this function. Can this form account for all possible cases? The German philosophers appear to believe it. I have, I avow it, some doubts in this respect; and besides, I am far from being convinced that the resistances, more or less great, which a current encounters in the circuit which it has to traverse, are the only causes of specific characters which it presents in its properties. I do not deny that these circumstances play a very important part in the production of these phenomena. In the memoir which I have already cited, in an anterior memoir, published in 1825,† and in almost all the researches that I have made since that time, I have shown the importance of taking these resistances into account. I thus explain the influence of the number and size of the pairs of of a pile, on the different effects which it is apt to produce;‡ I deduce from it the explication of the calorific effects of the voltaic current,§ &c. It was after having, at first, believed that the principle of resistances was sufficient to account for all these circumstances, that doubts began to arise in my mind, that I exposed in my letter to M. Arago, inserted in the *Annals de Chimie*,|| doubts which several experiments of Mr. Faraday, relative to the *intensity* of currents due to different chemical actions, appeared to me to confirm. I shall not enlarge further here upon this important point, which I intend to treat separately in a future part of this paper. I shall confine myself to the resumption of my thoughts, on the subject of the law of Ohm, in saying that it has not yet been proved to me that the principle of this law is perfectly sure; but that, even in

* *Ann. de Ch. et de Phys.*, t. xxxvii, p. 251.
† *Ann. de Ch. et de Phys.*, t. xxviii, p. 213.
‡ *Ann. de Ch. et de Phys.*, t. xxxix, p. 319, *et Memoires de la Soc. de Phys. et d'Hist. Nat.*, t. vi.
§ *Ann. de Ch. et de Phys.*, t. xl, p. 38.
|| *Ann. de Ch. et de Phys.*, t. xli, p. 38.

admitting it, there remains some doubts in my mind on the exactness of the form under which Ohm believed it to be possible to introduce, in the general expression of the intensity of a current, the circumstances which determine this intensity. These doubts are drawn up essentially in the difficulty of isolating, in an absolute manner, each of the determining circumstances, which it is necessary to do in order to know its part in the influence in the numerous anomalies which experiments present; especially when we act on energetic currents, which anomalies it is not always easy to explain; and, in fine, in the intimate connexion which I see exists between chemical action and electric action, and in the agreement which ought to result from it amongst the variations which these latter undergo, and those to which the first are subject.

After the general observations which I have just referred to, M. Leaz makes two remarks on the apparatus of which I have made use: the first is relative to the nature of the currents which I have studied, the other to the use I have made of the thermometer of Brequet, to measure the intensity of these currents by means of their calorific effect.

Here is, as far as I can comprehend, in what consists exactly the first remark. The author seems to believe that I attribute to each of the elementary currents, produced by induction, and not to their joint influence, the properties which I have sought to discover in magneto-electric currents. He has the air of supposing that I consider the origin of these currents, rather than the manner in which they are grouped, as the cause of the phenomena which they present; and he observes that, in consequence, I ought to have studied the effect of each current isolated, and not that of a succession, more or less rapid, of these same currents. It is this study which he himself makes, farther on, and of which the results are not, in point, in agreement with those which I have obtained, by occupying myself on the subject under another point of view.

I avow that I do not see why I ought to be obliged to occupy myself on isolated currents, rather than on them altogether. I have an apparatus (that of Clarke), which produces a series of electric currents by induction; I describe this apparatus: I give to the currents which it developes, a name (*magneto-electric currents*), which, without in any way prejudging of their properties, recalls to the mind the apparatus whence they emanate; I endeavour to analyse their effects; I expose the laws to which these effects appear to me to be subjected. Strictly speaking, I might stop short there, and say nothing of the cause to which I attribute these effects. However, in several parts of my memoir, I have explained myself on this point, and I have always declared an opinion contrary to that which M. Leaz seems to attribute to me, who has badly comprehended me, or at least, it appears so. I have said that I attribute the special properties of magneto-electrical currents, not to their being produced by magnets, but decidedly to the fact that they are

inconstant, and directed alternately in contrary directions.* If I have made use of a magnet, in order to procure my currents, it is because I have found that this was the most commodious means, and, at the same time, the most convenient to have them of a constant intensity. I have never denied that currents fulfilling all the same conditions, but produced by another source, do not possess the same properties.

The second preliminary remark of M. Lenz, has for its object, I have said, the employment that I made of the thermometer of Brequet, to measure the intensity of magneto-electric currents, by means of the elevation of temperature which these currents determine in the helix of this thermometer in traversing it. M. Lenz remarks, that the current is divided among the three plates of silver, gold, or platinum, which compose the helix, proportionally to their electric conductibility, and consequently unequally, that this current ought also to heat them unequally, so much as to cause this circumstance, since it produces more heat in proportion as the resistances which it meets with are greater. He concludes from it, that the phenomenon of the heating of a helix in Brequet's thermometer, by the electric currents, is a very complicated phenomenon, and that we cannot make use of this means to measure the intensity of currents.

I admit, with M. Lenz, that it is very possible that the current is not distributed uniformly among the three metallic plates which compose the helix; but I do not conclude from that, that the three plates have not exactly the same temperature. I remarked, at first, that the two circumstances, which are indicated as previously determining a different temperature in each plate, compensate the one for the other; for if, on one hand, the plate of less conducting power transmits less electricity; on the other hand, it becomes heated more by the action of the same current, by virtue even of the greater resistance which it presents. Besides, whatever be the mode of distribution of the current, it is impossible that the elevation of temperature which results from it is not the same in each contiguous

* This is what results from several passages of my memoir, especially the following:—Thus, after having described the apparatus of which I made use, I add: "We can, by an artifice, make one of the two currents disappear alternating contrary, in such a manner as only to have a series of currents all directed in the same direction; but they then lose a great part of their energy, and *some of their most remarkable properties.*" Here is a plain proof that I consider the fact that these currents are directed alternately in contrary directions, as the cause of some of their most remarkable properties." Afterwards, in speaking of the physiological effects, so called, of these currents, I say: "It appears that the power of these currents is essentially due to their inconstancy; for we know that, when a current acts in a continuous manner, whatever be its intensity, the animal submitted to its action only experiences commotions at the moment when this action commences, or at the instant it ceases. Farther, we can obtain the same effect in *rendering inconstant, by means of a very simple artifice, the current developed by a simple voltaic element.*"

part of the three plates; the three plates, taken conjointly, have only $\frac{1}{10}$ of a line of thickness, and how is it possible to believe that, during an experiment, which is continued always for some moments at least, the corresponding points of the three plates can maintain different temperatures? This would be contrary to all the phenomena of the conductibility for caloric, and the constant tendency of this agent to attain an equilibrium. Besides, it is of little importance; the electric current developes heat in traversing the helix of Brequet's thermometer, and I have found, in a memoir anterior to that now in question,* that the intensity of the current, concluded from the elevation of temperature which it determined in this helix, does not sensibly differ from that deduced from the indication of other galvanometric processes. M. Lenz will not probably have been aware of the memoir to which I have just referred.

But yet, I do not wish to conclude, from the preceding remarks, that the intensity of the current is exactly proportional to the indications of Brequet's thermometer; I have never made this pretension, and that the more, because I believe it would be very difficult to find an instrument whose indications would be in exact relation with the variations of the current. If I had made use of a metallic thermometer, it is on one hand, that I believe it to be useful, when studying electric currents, to examine the variations of their intensity, under the relation of all their effects; on the other hand, it was impossible to avail myself, in the study of that species of currents which I wished to analyse, of the chemical galvanometer, or of the magnetic galvanometer, because of the alternative non-interruption in the direction of the successive currents. In fact, with the chemical galvanometer, the two gases, which are the product of the decomposition of the water, arrive, almost simultaneously, on each of the platinum wires which serve as poles, and they are combined immediately, in great proportion, to recompose water; and with the magnetic galvanometer, the two alternative currents succeeding each other with great rapidity, the needle obeys, indifferently obeys, one or the other, following its position by relation to the wire of the instrument, or remains perfectly in equilibrio.

M. Lenz, in repeating and varying my experiments, made use of the magnetic needle; but then he has only employed a single instantaneous current, such as we obtain by detaching from a magnet a morsel of iron surrounded by a metallic wire in which a current is developed by induction. I do not hesitate to attribute the differences which exist between the results which he has obtained and those to which I was led, to the diversity of the currents which have separately been made use of by us. That M. Lenz finds it more interesting to determine the effects of a single current, nothing can be more legitimate. But, at the same time, I may be permitted to oc-

* *Mem. de la Soc. de Phys. et d'Hist. Nat.*, t. vii, p. 457, and *Ann. de Ch. et de Phys.*, t. lxii, p. 147.

cupy myself with phenomena which produce a succession of instantaneous currents alternately directed in contrary directions. I believe that this study may have important consequences, and I have commenced it; it is not by caprice, but for motives dated far back, and which have made me adopt with eagerness a simple and easy method of procuring a series of currents placed in the condition which I have above referred to.

(To be continued).

On the Extraction and Decolorization of Gelatine. *By* J. C. Booth *and* M. H. Boye.*

In a patent granted to S. G. Dordoy, of Surrey County, "for certain improvements in the manufacture of gelatine size and glue," (see *London Journal* for January, 1842), the patentee proposes the employment of chlorine, for bleaching the materials employed, previously to the extraction of the gelatine, in which respect the patent mainly differs from former attempts at bleaching by operating on the gelatinous fluid after its extraction.

"For every 100 lbs. of the animal substances, eight ounces of chlorate or chloride of lime, potash, soda, baryta, or other similar compounds are dissolved, or thoroughly mixed in, or with, two or more gallons of hot or cold water; four pounds of hydrochloric or other acids being added and stirred thoroughly. This mixture is to be poured into the vessel containing the water and animal substances, the materials being stirred continually while the mixture is added. The animal substances should be kept entirely covered with the water for twenty-four hours."

The above proportions are considered sufficient for thin skins, as of sheep, but for heavier pieces, such as those derived from oxen, calves, &c., two or three steepings will be requisite, each continued the same time, until the substances appear of an uniform, transparent whiteness. The substances are thoroughly washed in pure water at ordinary temperatures, and then water at 160° Fahr. is poured on, and the temperature maintained at 100° Fahr. from twelve to twenty-four hours, when the gelatinous solution is strained off. Fresh supplies of water are successively added, each portion at a temperature of 20° higher than the preceding, until at last the water is boiled.

This patent, like many others, makes a sweeping claim "to cover all the ground," but it might be restricted to the use of chlorine or its compounds with oxygen, for bleaching animal matters previous to the extraction of gelatine, and not subsequent to this process, which had been repeatedly tried. In the latter case it has been found that although the colour of the gelatine may be improved, its binding quality is materially impaired; but there is every reason to believe that the colouring matters may be obviated by the application

* *Journal of the Franklin Institute*, May, 1842.

of bleaching previous to extracting gelatine, since it is the opinion of chemists that this substance is not ready formed in the animal matters employed. Since, then, the patent contains good principles, it may be well to inquire into some of its details.

As is too often the case with chemical patents, its descriptions are vague and incompatible with the definite laws of chemistry. Thus it is not immaterial whether we employ the same amount of acid (of what strength ?) with the same quantity of chlorate of potassa, soda, lime, or baryta, or with the chlorides of the same bases. Probably the chloride of lime is the only compound that can be employed economically; and in this case, if, as the patentee directs, the acid, which is twice as much as necessary, be poured first into the solution of the chloride, a considerable amount of chlorous or bleaching material will be lost. It would be advisable either to dissolve the chloride in two or more gallons of cold water, and then add the acid previously diluted with three or more gallons of water, or to add the clearly drawn off solution of chloride to the water over the animal substances, and then to add the acid diluted largely with water. In either case, a bleaching liquor will be obtained which will act more uniformly and for a greater length of time without being exhausted.

The successive extraction of gelatine by water at low and increasing temperatures is worthy of notice, although not novel; but in this case the latter solutions should be employed for gelatine of decreasing qualities.

On the Preparation of Aluminous Mordants. *By* James C. Booth *and* M. H. Boye.*

Whatever relates to improvements in dyeing and cloth printing being of high importance, we have thought it worth calling attention to the above subject from the appearance of two patents, the one taken out in England by R. Hervey (*Rep. Pat. Inv.*, December, 1841), and the other in this country (*Jour. Frank. Inst.*, March, 1842). They are worthy of consideration from the comparative cheapness with which the aluminous mordants may be prepared.

The most important point is the formation of sulphate of alumina by the direct action of sulphuric acid upon clay which has been calcined in a reverberatory furnace. Supposing the process to be successful, as described by the patentee, it offers great advantages, since an abundance of clay comparatively free from iron may be obtained in many districts of country, and even a portion of that iron must be rendered insoluble by calcination. In one respect the sulphate of alumina may be more advantageously employed as a mordant or colour-base, than the sulphate of alumina and potassa (common alum), since a given weight of it will contain a larger proportion of colour-

* *Journal of the Franklin Institute*, May, 1842.

base than the same weight of alum; but, on the other hand, crystallized alum is so uniform in composition, that in its employment we operate more definitely and certainly, according to given weights, while the sulphate of alumina is difficult to be brought to a crystalline state. It is, moreover, liable to form basic salts, so that we cannot know with certainty and readiness the exact amount of alumina present in a solution of the salts. For these reasons the patentee is in error in supposing that the sulphate of alumina is superior to alum for printing, dyeing, &c.

The sulphate of alumina may be more successfully employed in the manufacture of the acetate, by means of acetate of lime or lead, for by decomposing alum perfectly, it is requisite to add a sufficient quantity of acetate to precipitate the whole of the sulphuric acid, not only that combined with alumina but also with the potassa, so that a considerable amount of the acetate is employed to no useful purpose; whereas for every proportion of the sulphate of alumina alone, that is decomposed by an acetate, an equivalent quantity of acetate of alumina is formed. The most economical method is to precipitate with *acetate of lime* as long as a precipitate is formed, and then to throw down the small balance of sulphuric acid by *acetate of lead.* The second method described in the English patent, of precipitating alumina by alkali, or its soluble carbonate, and then redissolving the alumina in acetic acid, may often be practised economically, but there is then some difficulty in obtaining a liquid of uniform strength.

The remarks which have been made apply to both the American and English patents, and particularly to the latter, which is more copious. Since each of the points claimed in the patents have been the subject of previous experiments and manufacture, it appears to us that it would be a difficult matter to sustain the combination, since other simple methods of removing the iron may be resorted to.

On the Application of Voltaic Electricity to Calico Printing. By Mr. Baggs.

Before entering into the details of the method of applying voltaic electricity to the printing of calico, and other textile fabrics, such as silk, paper, &c., it may be useful to give a brief sketch of the principles upon which calico printing, dying, &c., is at present carried on.

If we dissolve a small quantity of corrosive sublimate in a wine-glass of water, and add to this a little hydriodate of potassa, an immediate change is produced; and the two solutions, which in a separate state were perfectly clear and colourless, are instantly rendered opaque by the formation of a splendid scarlet pigment, the periodide of mercury, which gradually subsides to the bottom of the glass.

If, in this experiment, we substitute acetate of lead for corrosive

sublimate, the precipitate is a rich yellow; and so, by varying the metallic solution and the reagent employed in its precipitation, any desired colour may be obtained. In fact, it is by this means that colours are ordinarily formed for the use of the artist; and by an elaborate extension of the same principle, the calico printer is enabled to adorn the produce of the loom with his varied and beautiful devices.

It is the business of the manufacturer to select from the vast multitude of coloured compounds which his art can thus produce, such as are at once the most pleasing, economical, and permanent; for permanency of colour is here a most essential requisite. The dyes employed must be such as will be subsequently unaffected by washing in hot or cold water, and totally unalterable under all the casualties to which, in the course of fair wear, they are likely to be exposed. These several points are attained in a very simple manner.

The metallic solutions, or mordants, being previously inspissated with starch, gum, pipe-clay, or other appropriate thickener, are deposited in their respective places upon the cloth to be printed by means of a machine of great cost and complexity; indeed, its construction is so very intricate, that it would be difficult to attempt a description of its parts within the limits of this paper.

After the impression is thus given, the cotton is allowed a short time to dry. It is then immersed in the dye-bath, a vessel containing a suitable reagent. As the decoction penetrates the cloth it encounters the mordants, producing a number of coloured precipitates so closely entangled amongst its fibres, and incorporated with its substance, as to be perfectly proof against all subsequent washing.

The details of this branch of manufacture extend over so wide a field, and the niceties of manipulation required in its practice are so great, that a very brief and cursory notice must suffice.

The most important mordants in use are the acetate of alumina, the acetate of iron, and the protomuriate and permuriate of tin.

The colouring matter of cochineal is fixed by means of tin with the production of a scarlet, and by the addition of alum the scarlet is changed to crimson. A decoction of madder may be substituted for cochineal with but little inferiority in the result. A beautiful yellow is obtained by printing with acetate of lead, and subsequently immersing the cloth in bichromate of potassa. The ammoniacal solution of sulphuret of arsenic is also productive of a good yellow; so is a decoction of weld, of quercitron bark, of fustic, sumach, Persian berries, &c.;—the mordants in these cases being the salts of alumina and of tin. The first object of the chemist in every instance is to render the colouring matter *soluble*, so that it may thoroughly penetrate the pores of the cloth; and then, when the latter is perfectly saturated, to render it *insoluble*, and thereby fix it.

Blue is readily obtained by printing with an iron mordant, and afterwards passing the cloth through an acidulated solution of ferrocyanate of potassa; but indigo is more commonly employed for

this purpose. The drug is rendered soluble by depriving it of a portion of its oxygen, which element is again absorbed in the subsequent exposure of the printed fabric to the atmosphere; and thus the insolubility necessary to fixation is restored.

By diluting the mordants the depth of shade is varied, and by their appropriate admixture compound colours are obtained.

But quite enough has been said to give a general idea of the principles on which this art is founded, and those who seek more detailed information are referred to Dr. Ure's *Dictionary of Arts, Manufactures, and Mines,*—article, "Calico Printing." The subject is there treated in a very comprehensive manner, and will amply repay the trouble of a perusal.

We now proceed to discuss the new application of voltaic electricity to the purpose already stated. It would appear almost unnecessary, in the present advanced state of public intelligence, to enter into any description of the method adopted for the development of galvanic phenomena; but a few words on this subject may perhaps be acceptable to some of our readers.

If we take a plate of copper and a plate of zinc, and immerse them simultaneously in a vessel of dilute sulphuric acid, the zinc will immediately begin to be dissolved, and bubbles of hydrogen gas, resulting from the decomposition of the water, will be extricated from its surface; but no effect whatever will be produced upon the copper so long as the two metals do not touch each other: the moment, however, that they are brought into contact, nearly the whole of the hydrogen gas makes its appearance upon the surface of the copper, though the zinc is still the only metal which continues to be dissolved.

There is, in fact, a very extraordinary force thrown into circulation by the contact of these metals under the circumstances described. This force is termed galvanic, and in the line of its passage from the zinc to the acid, from the acid to the copper, and from the copper back again to the zinc, it produces a very singular class of effects. By combining together a number of these simple arrangements in consecutive order, so that the electricity generated by each pair of plates shall circulate through the whole series, the energy of the power which is thus called into action becomes highly exalted, and we have what is termed a compound voltaic battery. By attaching wires or slips of flexible metal to the two ends of the battery, a facility is afforded of transmitting a current of electricity through any body upon which we may wish to operate.

Suppose we pass this current through muriate of soda, or any other saline solution, decomposition takes place, accompanied by a definite transfer of the resulting elements to the two poles or terminating wires of the battery. The hydrogen of the water and the alkali of the salt make their appearance at what is termed the negative pole,* and the oxygen and the acid at the positive pole; and if

* In a subject of this nature the introduction of some few technicalities is almost unavoidable; and their explanation is, at the same time, impossible in so limited a paper.

the metal forming this latter have a moderate affinity for oxygen, it will be chemically acted upon and dissolved. The extension of this one fact is all that is requisite for the production of the most elaborate designs by voltaic electricity.

Let it be required to print in two colours, blue and brown. A compound pattern must be formed in such a manner as to present to the cloth, or other substance to receive the impression, different metals in different parts of its surface. The metals in this case would be iron and copper, and the mode of printing such a pattern will be readily understood from the following description:—

Upon a smooth plate of metal, in connexion with the negative pole of an active battery, two or three thicknesses of calico are to be placed, having been previously moistened with a mixed mordant of nitrate of soda and ferrocyanate of potassa. On the calico is laid the metallic design. This contact, however, produces no effect until the upper plate is touched with the positive wire; but the moment the electric circuit is thus completed, a decomposition of the interposed solution takes place; hydrogen, potassa, and soda pass to the negative pole, whilst oxygen, with nitric and ferrocyanic acids, are disengaged at the positive pole; where, acting upon the metals thus presented to them, an accurate copy of the design, in its proper colours, is instantaneously produced. The cause of this is obvious: ferrocyanate of iron is blue, and ferrocyanate of copper is brown, and thus the two pigments required are produced wherever the two metals touch the cloth.

The introduction of nitrate of soda into the mordant is for the purpose of facilitating the passage of the electricity, and of preventing the incrustation of insoluble matter upon the metals, which, without this aid, would inevitably take place, and totally check the operation after two or three impressions.

But let us take another example.

To print in red and black, the cloth must be soaked in an aluminous mordant, and the design made of one metal alone—iron. This is touched with the positive pole of the apparatus, as in the instance just given; and, after the current of electricity has passed through the cloth, the latter is to be immersed in a decoction of madder. Wherever the pattern comes in contact with the cloth there will be a black impression developed, whilst the remaining portions will be dyed red.

Discharges may also be produced by this means, or a topical alteration of colour obtained when required.

If a zinc plate be placed upon calico, already dyed with Prussian blue and moistened with nitrate of soda, and lying on the positive pole of a battery, the moment the zinc is touched with the negative wire the blue is changed to a beautiful brown in those parts of the cloth which transmit the electricity; for the alkali no sooner reaches the negative plate than it decomposes the Prussian blue, taking up the acid, and precipitating peroxide of iron.

A great variety of these effects may be obtained. Cloth dyed

with indigo, and moistened with a solution of common salt, slightly acidulated with muriatic acid, is bleached by the battery at the positive pole, which, in this case, should be made of platina.

It must not be forgotten that in conducting these experiments it is necessary to use starch, or some other thickener, with the mordants, as otherwise the colours would run.

The principles which have been advanced and exemplified in this brief account, are susceptible of considerable modification; and when the light of science has been brought to bear more fully upon the subject, it is confidently anticipated that a wide field of application will disclose itself in our arts and manufactures. Electricity may possibly entirely dispense with that complicated machinery at present required in the process of topical dyeing, and the many tedious operations attendant upon the system now in use will be considerably abridged by the introduction of this wonderful agent.

NOTE.—Mr. Baggs, the discoverer of this beautiful application of voltaic electricity, during a late visit to Manchester, very obligingly presented me with patterns in several kinds of metal, necessary for illustrating this mode of printing; and he also performed several successful experiments, in the Lecture Room of the Royal Victoria Gallery, which I have since availed myself of in several lectures on this branch of electricity. Since Mr. Baggs left Manchester, I have been successful in applying magnetic electricity to the same purpose; and I find, also, that sparks from the prime conductor of a common machine will produce some of the colours.—W. STURGEON.

Hints on a New Theory of the Phenomena of the Daguerreotype. *By* J. RYAN, ESQ., M.D., &c., &c.*

IT is a source of much regret to the scientific world that the *rationale* of an art, which seems almost to realize in our days the Promethean fable of the ancients, should remain so long in obscurity.

We are well aware that numerous hypotheses have been advanced, both by our continental neighbours and by our own countrymen, in order to account for the phenomena of the daguerreotype; but, at the same time, we have to confess our conviction of the futility of each and all which have hitherto been brought forward.

While any art, whatever may be its aim and character, remains in a state of empiricism, we cannot expect any very rapid or marked progression. Each step in advance is taken in the dark, amid uncertainty and fear; and ultimate success, whether immediate or remote, is the result of chance rather than of induction.

We are not ignorant of the avowal of some, that a condition like this is favourable rather than otherwise to the success of an art or science, giving a stimulus to repeated experiment, and preventing

* *Polytechnic Journal*, March, 1842.

the substitution of theory for practical result. But our readers will agree with us, that while it would be wrong to build up theories and then torture our experiments into an accordance with our preconceived notions, yet the true philosophic mode is to pass, step by step, from experiment to experiment, by the light of inductive reasoning; to aggregate facts, certainly, but not to rest content until we have so classified and arranged, and investigated them, as to leave no part of our process a matter of doubt. Until this is accomplished, any art or science is in a state of illegitimacy, having no claim to the honourable distinctions of true philosophy.

Our readers are aware that the favourite theory of the daguerreotype is at present the chemical one, founded upon a pure hypothesis—namely, "that the gold-coloured pellicle, formed by the vapour of iodine upon the silver plate, is a compound of iodine and silver." M. Donne and others contend, that the chemical action between the silver and iodine, results simply in the production of the common iodide of silver. This, however, will not bear the tést of experiment; and if there be chemical action at all, the compound resulting is possessed of properties not appertaining to the well known iodide.

The next step in the chemical theory is the decomposition of this newly formed compound of iodine and silver by means of light; so that wherever the light has fallen the silver surface becomes exposed, and prepared for the action of the mercurial vapour in the third step of the process. "This explains," says M. Donne, in his letter to M. Arago, "the extreme inconvenience, pointed out by M. Daguerre, of leaving the silver plate too long exposed to the vapour of iodine—until, for instance, a violet tint be produced; indeed, in such case, two couches of iodine are formed, the one of a violet colour and superficial, the other more solid and yellow. Thus, when the light has acted on the first, it does not affect the second, and the latter does not admit of the action of the mercury. This fact may be confirmed by removing with the finger the first couch of iodine acted on by the light, below which presents itself the couch of yellow, intact. According to these experiments, the image produced by the process of the daguerreotype would be thus formed—the light parts by the mercury in globules, and probably amalgamated with the silver; and the shadows by the simple metallic surface, without any foreign deposit or production of any combination. It is, in fact, that which is left—when, after the operation, all remaining traces of the iodide have been removed by a wash of the solution of the hyposulphite of soda. The black or shaded parts are bare, and reflect the light like polished bodies and glasses, while the light parts are covered with a couch of greyish white, which soils the fingers, is easily removed, and in which the microscope exhibits a quantity of mercurial globules: hence the necessity of a perfect burnish of silver, as M. Daguerre recommends."

The chemical theory, of which the above is a condensed, yet fair view, is wanting in two very material points. In the first place, it

is founded upon a simple hypothesis; and secondly, it fails to account, in any way, for the cause of the action of light upon the iodide of silver—if such it be. At any rate, it has to set at rest the long-disputed point of the existence of chemical rays of light, ere we can admit the decomposition of the new compound by light.

A highly valued friend* of the writer has lately put him in possession of some new hints on the theory of the daguerreotype, and as they are based upon well known data, and seem worthy of further consideration, he has ventured to bring them before the public eye, leaving the further investigation of this important subject to those who have sufficient opportunity for that purpose. The propositions embodying the new theory have been in the possession of the writer for some months, but owing to the multiplicity of his engagements, he has been hitherto unable to follow out by experiment, as he once intended, the valuable hints therein embodied.

The phenomena of the daguerreotype (says Mr. Baggs), are evidently referable to electricity; and in proceeding to offer an explanation of the subject, I prefer the adoption of that line of argument which is the most open to contradiction, if any trace of sophistry should be manifested. The apparent *rationale* is, therefore, embodied in a series of progressive propositions, every one of which will, I hope, bear the test of the closest investigation. Wherever I have deemed it necessary, I have quoted an authority in support of my propositions; but where the facts are so well known or so self-evident as to render this unnecessary, I have, of course, spared myself the trouble of doing so. Thus, of the three first theorems, I would only remark, that they are contained in every elementary work on electricity, as facts long and sufficiently established.

PROPOSITION I.

Bodies, *dissimilarly* electrified, attract each other.

PROPOSITION II.

Bodies, *similarly* electrified, repel each other.

PROPOSITION III.

Positive electricity can only be neutralized by the addition of *negative electricity*, and conversely, *negative* by *positive*.

PROPOSITION IV.

The formation of vapour is invariably accompanied by the development of electricity.

"If a hot plate of metal be placed upon a gold-leaf electroscope, and water be dropped upon the plate, the moment the vapour rises the leaves of the electroscope diverge with negative electricity.

"The general fact was noticed by Laplace, Lavoisier, and Volta,

* Isham Baggs, Esq., the discoverer of the method of printing by electricity, models of whose apparatus may be seen at the Royal Polytechnic Institution.

in the year 1781, and was found to extend both to solids and liquids passing into a gaseous form."*

PROPOSITION V.

Light is capable of conferring an attractive power upon glass.

"If a glass jar, containing camphor, be placed in a window, beautiful crystals will attach themselves to the sides nearest to the light. Evidence of this fact may frequently be seen in druggists' shops. Many other substances, which are capable of the same kind of sublimation, exhibit the same phenomena in their solidification."†

M. Chaptal first made the observation—

"That when a number of capillary crystals shoot up the sides of a glass vessel containing a saline solution, they attach themselves only to that side of the vessel which is most strongly illuminated. He was thus able to cause crystals to form on any selected side; and by placing a screen before the vessel he found that the line between light and darkness was distinctly marked by the limit of crystallization. This result is most readily obtained with the metallic salts."‡

Let us now ascertain how far these several facts, embodied in the foregoing propositions, will serve to explain the phenomena of the daguerreotype; and in order to do this, we must follow the process from its very commencement.

The first step in our process, then, is to prepare a highly polished silver tablet, which is a *conductor* of electricity. This prepared tablet is then placed in a box containing a small quantity of iodine, "a *non-conductor* of electricity."§

The vapour of iodine rises; and we know, by the fact embodied in our fourth proposition, that it is charged with electricity. The first film of iodine which is deposited upon the plate *loses* its electricity by coming in contact with a conducting surface of silver; but the after portions of condensed vapour *retain* their electricity, for they do not fall upon the conducting silver, but upon the film of non-conducting iodine, which has just been deposited.

The result, then, of the first operation is the production upon the silver tablet of a non-conducting surface, the upper portions of which are charged with electricity.

Now, instead of at once transferring this prepared plate to the camera, let it be submitted to the vapour of mercury in the ordinary way—but no mercury will adhere to it. The conclusion is irresistible. The vapour of mercury we know to be electrified (proposition iv), and its electricity is evidently similar to that of the prepared tablet; therefore it is repelled by it (proposition ii).

Let us now return to the generally adopted process:

The plate is placed in the focus of a camera obscura, where it remains exposed to the action of light for a certain period of time. It

* Dr. Roget's *Treatise on Electricity*, page 55.
† Daniel's *Elements of Chemical Philosophy*, page 399.
‡ Daniel's *Introduction*: before quoted, page 399.
§ Brande's *Chemistry*, vol. i, page 155.

is then submitted to the vapour of mercury, not, however, with the same result as before. The mercury is now no longer repelled by those portions of the tablet which have been submitted to a strong light, but is attracted and accumulated there in exact proportion to the degree of action which has taken place at these several points. The plate may now be exposed with safety to the light, even before the coating is washed away; for, as we do not wish again to expose it to the vapour of mercury, the electrical condition of its surface is perfectly immaterial.* From a consideration of the several points enumerated above, it follows, that when the charged plate is exposed to the action of light, its electricity is *lowered*, or *totally destroyed*, in the illuminated parts, and its repulsion to the vapour of mercury no longer maintained. It would appear, too, from the foregoing propositions, that *light* itself is either the same with electricity, or that it has the power of exciting that agency in the bodies upon which it falls; for, by our third proposition, we find that "positive electricity can only be neutralized by negative, and negative by positive." If therefore, we once determine the nature of the electricity of the vapour of iodine or of mercury, we can readily ascertain that of light, for it must be of the opposite kind.

Such then, given in his own words, are Mr. Baggs's views of the phenomena of the daguerreotype. That the propositions upon which his reasoning is founded are correct and well established, no one will for a moment doubt. The question, therefore, is, are the deductions from the facts embodied in the data sound and logical, and will they bear the test of close and accurate experiment? If Mr. Baggs's theory be correct, much will be accomplished in the elucidation of some of the most profound mysteries of science. We wish he would still continue his researches in this important matter, nor rest content with the mere outworks of the question. We would venture to remind him of the words placed by Juvenal in the mouth of the one-eyed Hannibal when he had overcome the rugged passes of the Alps, and already stood upon the plains of Italy.

"Actum, inquit, nihil est nisi Pœno milite portas
Frangimus, et media vexillum pono suburra."

February, 1842.

EARTHQUAKES, ELECTRIC STORMS, ETC.

Terrific Thunder Storm in Ireland.

On Sunday morning, Feb. 9, this town and the vicinity was visited with one of the most terrific thunder and lightning storms recollected even by the very oldest inhabitants. About seven

* In detailing the steps of the process, Mr. Baggs, for the sake of simplicity, merely refers to the employment of iodine as the sensitive material, that being the substance originally used by Daguerre. The employment of the sensitive compound of chlorine and iodine, however, could not at all affect the truth of the author's propositions.

o'clock, A.M., the wind commenced blowing a perfect hurricane from the N.N.W., the rain and large hailstones descending in torrents, and deluging the face of the country. Loud peals of thunder and vivid flashes of lightning followed in close succession, and continued for nearly five hours. Several houses in Milebush were blown down; and the electric fluid killed half a dozen sheep and pigs in the parish of Breaffy. Throughout different parts of the country we understand that the injury done to cattle and poultry has been very considerable; but happily, so far as we have been able to ascertain, there was no destruction of human life. During the continuance of the gale, it blew more powerfully than during the memorable storm of January, 1839.—*Mayo Telegraph.*

An Earthquake in Cornwall.

A report has been drawn up by Mr. Hunt, for the Polytechnic Society, of the particulars of the earthquake felt in Cornwall, on the 17th of February last; from which we collect the following particulars:—In the neighbourhood of Falmouth the noise attracted attention, although but few persons felt any tremulous motion. At Penryn the disturbance was more decidedly felt, doors were shaken and the earthenware rattled on the shelves. At Roosanooth, and down the valley, the shock was considerable, and the first impression was that the powder mills in the neighbourhood had exploded. The inhabitants of the villages of Comfort and Lammer, about the junction of the granite and killas, or clay slate, left their houses, thinking that some serious explosion had occurred at the neighbouring mine. At Cann Marth, a widow woman ran out shrieking that the side of her house was falling in. In Poldory, the shock was felt by the men working 130 fathoms below the surface. In other places it was felt even more intensely; and at Constantine doors flew open, persons were shaken in their beds, and some men working in the house ran out to know what was the matter. At Helstone the disturbance was considerable. At West Wheal mine, men at work in the 100 fathom level felt a shock, and, as it were, a rush of air; so much so that one of the candles was put out by it, accompanied by a noise which made them think that one of the shafts had crushed in. This appears to have been the most westerly part at which the tremor was felt, although the noise was heard further, but in diminished force. Mr. Hunt states that, on referring to a geological map of the county, it will be found that the greatest effects were produced near the edge of the granite mass, which extends from the north-east to the south-west; from Cann Marth to the south of Penryn. Although it was felt at Falmouth, Helstone, and other places, which are on the clay slate, yet all his inquiries went to show that it diminished rapidly in force as the distance from the granite increased.

A Factory Chimney Struck by Lightning.

During the thunder storm which visited this neighbourhood, on Sunday afternoon, Feb. 27, a chimney, in the yard of the cotton mill of Messrs. Carr and Co., Heaton Lane, Stockport, was struck by lightning. The chimney, which is octagonal and of recent erection, stands detached in the middle of the factory yard, and is about seventy yards high. The electric fluid broke the cornice of the chimney, taking with it a number of the inside and outside bricks, and descended down an external angle, tearing the brickwork from the fabric to the breadth of fifteen or twenty inches, and eight or ten yards in length. For eight or ten yards lower down, the chimney is not perceptibly injured; but at that distance the outside brickwork is again torn off, perhaps to the breadth of two feet, and a yard and a half or two yards in length. Seven or eight yards lower down, but not in a parallel line, is a somewhat similar fracture, after which no other damage is perceptible on the outside. What may be the injury to the inside of the chimney has not yet been ascertained, it not having yet been examined. The occurrence will not throw any hands out of employ during repairs, as the mill has been closed several months.

Destructive Earthquake in St. Domingo.

The following account of a most frightful and destructive earthquake in the island of St. Domingo is given by the *New York Morning Courier*, from a Port-au-Prince paper of the 11th of May :—

On Saturday, the 7th inst., at twenty minutes past seven in the evening, we experienced some severe shocks of an earthquake, which put the whole town in commotion. At midnight a thick meteor was perceived passing to the east. The heat was excessive, and thick clouds hung over the neighbouring mountain, going in a direction from the south-east to the north-east. The seamen who were in the roads report that they felt the shock before they saw the houses shake, which would indicate that the shock came from the east. Many persons, however, think that the oscillations came from the north and went south. There were two very decided shocks, the first was not so long as the second; the latter was the most violent, and lasted about three minutes. All abandoned their houses, and the streets were filled with the affrighted population. But a little more, and Port-au-Prince would have been the scene of a disaster similar to that of 1770, a fatal year, which occurred to the minds of every one. There is scarcely a single brick or stone house which has not suffered damage; they are all more or less damaged. Some, it is said, are scarcely habitable. The facade of the Senate House, on which is sculptured the arms of the republic, surmounted by the tree of liberty, were detached from the

edifice, and broken into pieces by the fall. The interior of the building has also received some damage. In the night, between Saturday and Sunday, two shocks were again felt; but not as violent as the first; one at 10 o'clock, and the other at 12. At 11 o'clock in the morning of Sunday came another shock: mass at church was interrupted, and those officiating at it ran away, and many females fainted. Monday, at 11 o'clock in the morning, another shock. The weather, during these three days, had a lowering, and, at times, a threatening appearance. Monday evening, a little rain, with excessive heat before and afterwards; night cool. Tuesday, a change of weather; return of the breeze and appearance of rain. In the evening, at eight o'clock, the weather was stormy, and every thing seemed to indicate abundance of rain. The hopes we entertained yesterday have not been realized. On Wednesday we were awoke at a few minutes before five in the morning by another earthquake. During these latter days it appears to us as if the earth on which we were walking was constantly quaking.

SAINT MARC.—A letter from this town, which has been communicated to us, informs us that there, too, the earthquake of Saturday last was felt with the greatest violence; many houses have been so much shaken that they threaten every instant to fall down. On some plantations, in the neighbourhood of the town very great damage has been done.

GONAIVES, MAY 8.—Yesterday afternoon an earthquake was felt in this city, which was so violent that most of the houses in it were thrown down. At the same time, in consequence of the shock, a fire broke out in the apothecary's shop of Mr. Invernezzes, and consumed, in a few moments, an entire block. The flames destroyed everything that came in their way; there was not a drop of water in the town. All the houses which have not been burnt down have been injured by the earthquakes, and this morning the shocks occur every quarter of an hour. The shops of Madame John Joufferts and M. Dupuy have fallen a prey to the flames. The shops of M. Richard Dauphin and M. Oster, built of stone and brick, have fallen down. Houses and shops are inaccessible, and we write these hurried lines in the street. The whole population has passed the night in the middle of the streets. Of the merchandise, which the merchants had been obliged to pile up in the public square, a great part has been stolen. It is impossible at present to estimate the extent of the loss. The church, the prison, the national palace, the treasury, the arsenal, and the house which was getting ready for the colonel commanding this district, are now nothing more than a heap of ruins. In short, no one has escaped the calamity. Now, while we are writing, the fire is entirely extinguished; but the sky looks threatening, and we are afraid of more shocks. If, unfortunately, our fears should be realised, there will be an end of the few houses remaining standing, and Gonaives will be no more. The first and principal shock lasted about five minutes, and was followed, during

the night, by more than twenty others, which, though not so violent, were equally fearful. It is now eight o'clock in the morning. Not half an hour has passed since we had another violent shock. The number of persons killed and wounded is not yet known. All the prisoners who were not buried under the ruins of the prison have escaped. God grant that the capital may not have been afflicted with a similar misfortune.

CAPE HAITEN, WEDNESDAY, SIX O'CLOCK IN THE EVENING.—Most deplorable news is spreading throughout the city. It has been brought by Mr. Obas, son of the general commanding the district of Plaisance. In consequence of the earthquake which was felt here on Saturday evening, *Cape Town has entirely disappeared, and with it two-thirds of the population.* The families which escaped this disaster have taken refuge at La Fosette, where they are without shelter, clothes, or provisions. 'Such is the news circulating in town, and which unfortunately is probably too true. It is to be hoped, however, it will not be confirmed in its full extent. It is said that the president of Hayti has given orders to the physicians and officers of health attached to the hospital, to set off this evening, and give their assistance to the unfortunate victims of this disaster. Captain Morris (of the brig *William Nelson*, which brings the account) states, in addition, that a few hours previous to his departure, a courier arrived with information that at Cape Haiten a fire succeeded the earthquake, destroying the remaining houses, the powder magazine, and the remnant of the inhabitants. St. Nicholas and Port Paix are said to be in ruins, and in fact all the towns on the north side of the island. According to a letter in the *Journal of Commerce*, but one inhabitant of the Cape, a Mr. Dupuy, was saved, all the rest being either crushed, or drowned by the sea, which rose and submerged the city. Fearful, fearful indeed, are the particulars of this awful visitation.

Terrific Thunder-storm.

On Wednesday afternoon, May 27, the town of Birmingham was visited by one of the most terrific thunder-storms that we have witnessed for some time past. Fortunately, no loss of life has been occasioned by it; but a circumstance occurred in Coleshill-street, which caused the greatest consternation in the neighbourhood. While Mrs. Spiers, the landlady of the Green Man Tavern, was standing at the window with her servant, they both received a shock at the back of the head, as though some person had struck them. At the same time, the room appeared to be enveloped in smoke and flame, a strong smell of sulphur pervaded the apartment, the servant was knocked down, and a part of the window-frame was violently displaced. But, in the upper part of the house, the effects produced were even more singular. A strong oak door-post was shivered

into small strips resembling laths, and many of these pieces were conveyed to the distance of several yards. The window-frame was, in this instance, forced inwards, the bell-wires were partly fused or else broken up into short lengths, and marks were observed in different parts of the house, as though the electric fluid had singed it in the course of its rapid transit.—*Birmingham Gazette.*

Thunder and Hail-storm, near Retford.

During the evening of Friday last, May 29, between seven and eight o'clock, a severe storm of thunder, lightning, and hail, passed over the village of Mattersea, and the adjoining villages, near Retford, which did considerable damage. The thunder and lightning were extremely awful; but the injury done to vegetation was by hail, which lay from two to three inches in thickness, and consisted more of large and irregularly formed pieces of ice, than of common hail; and such was its density, that on the following morning it lay exposed in large masses under the hedges in every direction. The damage done to the gardens is very considerable, especially to the fruit trees; but every part of the vegetable world has suffered severely by this, happily, not very frequent occurrence in this part of the country.—*Doncaster Gazette.*

The Weather.

The heaviest rain and the most appalling thunder and lightning experienced at Stamford for many years, occurred between one and two o'clock last Tuesday afternoon, June 14. The day had been extremely sultry, and the storm came on suddenly from the west. The rain descended in such torrents as made many of the streets resemble the channels of rapid rivers, and flooded houses in low situations to the depth of two or three feet. Even those in elevated parts of the town were also damaged, by the insufficiency of ordinary spouting to carry off the water from parapets and cisterns, and from this cause general consternation was produced; but the alarm was fearfully augmented by the lightning, succeeded by thunder of that tremendously loud, continued, and crackling kind, which is understood to imply the presence of the greatest danger from such visitations. About half-past one o'clock, the electric fluid struck the beautiful spire of St. Mary's Church, built in the year 1260, and considerably damaged its highest part, forcing some feet of the stonework out of the perpendicular, and also striking off large pieces of the ornamental parts from two of the upper windows. Several of the stones fell into the street, near Standwell's Hotel, and others were afterwards picked up in the belfry. It is feared that the damage done to the steeple is considerable, and that very costly re-

pairs will be necessary. Passing onward to the north-east, the lightning struck an attic window in the house of Mr. Mortlock, bookseller, smashing five panes of glass, and rending the frame-work to shivers. It then greatly damaged a stack of chimneys of the next house, occupied by Mr. Althorp, druggist, and pierced through the slated roof into a closet containing a large quantity of Congreve matches in boxes: happily these combustibles were not ignited, or the consequences might have been dreadful; some of the boxes containing the matches were actually thrown to the ground from shelves on which they had stood, but their contents were not deranged.—*Stamford Mercury.*

NOTICES OF NEW BOOKS.

Dictionary of the Arts, Sciences, and Manufactures; Illustrated by Eleven Hundred Engravings. By G. FRANCIS, F.L.S.; Author of *The Analysis of British Ferns, The Little English Flora, The Grammar of Botany,* &c.—BRITTAIN: Paternoster-row, London.

WE have looked over Mr. Francis's *Dictionary of the Arts, Sciences, and Manufactures,* with a great deal of pleasure, because we find it to be a book of extensive usefulness, and one that was much wanted. It is, indeed, the only one of the kind that we have hitherto met with, and we are inclined to think that there is no other of its kind in the English language. It contains an immense number of technicalities which are of continual occurrence to readers of scientific works: many of which, being of recent birth, are not to be found explained in any other work. Mr. Francis, being himself well initiated in the principles of science, and an extensive reader of scientific works, as well as being an investigator and teacher, is well qualified for such an undertaking; and, we are perfectly aware that, independently of such qualifications, no author could have produced a work of such high interest, to the generality of scientific readers, as that now before us. Every technical word and phrase in the book is clearly and fully explained; and upwards of a thousand of them are illustrated by well-finished engravings. Every reader of scientific works ought to have this dictionary at his elbow.

Mr. Francis is also bringing forward another work of great interest, which we shall notice more particularly in our next number.

Letter from William Sturgeon *to* Professor Silliman.

My dear Sir,

In looking over your excellent journal for April last, I find a letter inserted from a London correspondent, to whom I ought to be much obliged for acknowledging, though reluctantly, that *I* was the discoverer of the remarkable fact, that *the extremity of the positive wire of a voltaic battery becomes red hot outside of the electric circuit.* As your correspondent has neither laid claim to any participation in the experiments, nor contradicted any of my statements on the subject, a perfect stranger to the man, and to the hitherto unpublished transactions connected with that affair, and to some others between me and the London Electrical Society, would be at a loss to understand the motives which induced him to address you; unless, indeed, they could perceive, as possibly they might, a disposition evinced to laud Mr. Gassiot's *liberality*—to blame me for not selling my "*right* for a mess of pottage,"—and a decided hatred to the pronoun I.

Your correspondent says, "I am surprised that in a *joint-stock undertaking* like this, he (W. S.) should talk of *his* experiments as distinct from those of the rest, but more so when these were kept secret from us." This is "We blew the bellows," in earnest. With the exception of the peculiar distribution of the battery for the decomposition of water, which was solely devised by Mr. Mason, there were no experiments on the occasion but those which were projected and carried into execution by myself; neither have I heard of any discovery being made with that battery since the time that I left the "joint-stock" business, of the proceedings of which, I got sufficient experience for a whole life-time in a very few meetings. These facts, I hope, are sufficient reasons for my declining to bring forward any other experiments in the *joint-stock undertaking;* and had there been no other demonstration, the whole tenour of your correspondent's letter would have amply proved that I was not mistaken in the views which were then forced upon me, of the probable consequences of a continuation of my humble labours, where even credit alone was already too great a boon for them.

In conclusion, permit me to exonerate you from any blame which you may suspect of having fallen into by the publication of my letters. I write no secrets: which practice, I believe, is my prevailing sin. And, when you read a series of documents, which I shall now be induced to publish, I think you will not blame me for making either hasty claims to discovery, or hasty disclosures of uncourteous treatment.

I am, dear Sir,
Your's truly,
William Sturgeon.

Royal Victoria Gallery of Practical Science, Manchester,
June 20th, 1842.

To Professor Silliman.

MEETING OF THE BRITISH ASSOCIATION IN MANCHESTER.

FIRST MEETING OF THE GENERAL COMMITTEE.

The general committee held its first meeting in the lecture theatre of the Royal Institution, at one o'clock on Wednesday, June 22nd, for the election of sectional officers and the transaction of general business, when there was a numerous attendance of members. The Rev. Professor Whewell, of Cambridge, occupied the chair. Amongst those present we observed our venerable townsman, Dr. Dalton, who appeared to take great interest in the proceedings.

Professor John Phillips read the minutes of the two last meetings of the general committee, which were confirmed.

The Chairman observed, that in order to induce men of science and distinction to attend the meeting, letters had been written to various foreign bodies, the object of which was the prosecution of science, inviting them to send men of science here, and informing them that every accommodation would be provided for them. Although some distinguished foreigners would be present, yet some of those societies had deputed countrymen of our own to represent them, and he believed Dr. Faraday could speak on behalf of one body.

Dr. Faraday said he had the honour of appearing before the meeting as the representative of the Academy of Sciences at Modena, the members of which had deputed him to return thanks on their behalf for the kind invitation which had been forwarded to them by the committee of the British Association; and he was authorised, at the same time, to express the great desire felt by those whom he represented to co-operate and do everything in their power for the advancement of science. Sir John Herschel was combined with him in this matter, whom he hoped to have had the pleasure of seeing present.

The Chairman: The next proceeding is the report of the general council, which manages the affairs of the society in the interval between the general meetings, and which prepares the report which the secretary will now read.

Lieut.-Colonel Sabine, F.R.S., one of the general secretaries, then read the following report:—

REPORT OF THE COUNCIL TO THE GENERAL COMMITTEE.

1. The council having learnt that it had been the intention of the geological section, at the meeting at Plymouth, to have proposed to the committee of recommendations that Mr. Edward Forbes should be requested to draw up a report on the *radiata* and *mollusca* of the Ægean and Red Seas, and that £60 should be placed at his disposal for expenses connected therewith—but that, from a circumstance purely accidental, this proposition had not been submitted to the committee of recommendations, and being also informed that Mr.

Edward Forbes was then in the localities referred to, and willing, if requested, to prosecute the researches, and furnish the report in question—the committee passed the following resolution:—"That Mr. Edward Forbes be requested to draw up, for the British Association, a report on the *radiata* and *mollusca* of the Ægean and Red Seas, with special reference to the relation of the recent genera and species of those which have hitherto been supposed to exist only in a fossil state." With regard to the grant of £60, which it had been the intention of the sectional committee to have recommended, the council were of opinion that the exigency would be provided for in the least exceptionable manner by requesting the treasurer to place that sum at Mr. Forbes's disposal, on the guarantee of individual members of the council that the amount should be made good by private subscription among themselves, in the event of that grant failing to pass the committee of recommendations or the general committee, when it should be brought forward at Manchester.

2. With the view of securing early attention to an important subject, the council requested the following gentlemen, who were stated as willing to act together for the purpose, to consider the rules by which the nomenclature of zoology might be established on a uniform and permanent basis, and to report thereon to section D, at the meeting at Manchester:—Mr. Darwin, Professor Henslow, Rev. N. Jenyns, Mr. Ogilvy, Mr. J. Phillips, Dr. Richardson, Mr. Strickland, and Mr. Westwood.

3. The council requested Dr. Lamont, of Munich, corresponding member of the British Association, to draw up a report upon the system of meteorological observations commenced in Germany, the report to be presented at the present meeting.

4. The council have added the names of Dr. Ritter, of Berlin; Professor Boguslawski, of Breslaw, and Professor Wartman, of Lausanne, to the list of corresponding members of the British Association.

5. The council have considered the proposition referred to them by the general committees for admitting to the meetings the children of members at the reduced price of one pound per ticket, but from information which has come before them during the preparations for the present meeting, they have been led to believe, that instead of the more limited plan for the accommodation of a few young persons, an arrangement might be preferable by which individuals generally, of all ages, are likely to derive benefit from, or give assistance in, the sectional meetings, who may be allowed to attend those meetings only on the nomination of a member, paying for such privilege £1, and receiving an appropriate ticket, distinct from that of the members. The council, therefore, recommend to the general committee the expediency of permitting this plan to be put in operation provisionally for the present week, and that the propriety of its being continued as a rule of the association be determined by the experience of its working on the present occasion.

6. The time allotted for the sitting of the sections having been found at several of the later meetings insufficient in the case of some of the sections, for due attention to be given to various papers and subjects brought before them, and the useful discussions arising thereon having been necessarily, and sometimes very unsatisfactorily, contracted in consequence, the council have been led to suggest the expediency of any particular section so circumstanced holding an evening meeting by adjournment. By an occasional recourse to such a practice, the council are of opinion that the very important purposes of the sectional meetings may be materially furthered, without trenching unduly on the opportunities, also extremely valuable, of conversational intercourse amongst the members of the assembly generally. In cases also where the subject to be brought forward is of interest to a wider circle than that of the section to which it more particularly belongs, such evening meetings would afford to the members of other sections the opportunity of being present without interfering with their attendance at their own section.

Should this suggestion be sanctioned by the approval of the general committee, the council are of opinion that such general meetings might be arranged without difficulty by the presidents of sections, acting conjointly with the officers of the association.

7. It has been notified to the council, that a deputation has been appointed to present to the British Association, at Manchester, an invitation to hold the meeting of 1843 at York; and that a deputation would also attend, on the part of the citizens of Cork, to invite the association to hold the meeting of 1843 in that city.

The council had been informed that on a proper application to her Majesty, the Queen might be graciously pleased to place at the disposal of the British Association, to be used for scientific purposes, the building in Richmond Park, formerly occupied as the Royal Observatory, but recently dismantled; and deeming that the possession of that building might materially promote the objects of the association, by the facilities which it would afford for the prosecution of experimental inquiries in the physical sciences, for which its locality is peculiarly suitable, as a place of reception for instruments and apparatus, for which grants might have been, and might hereafter be made by the British Association, and which, for general and special reasons, it may be desirable to place in it for use or preservation: also as a depository for the books or other property of the association—required the president and general secretaries to take the necessary steps for securing this important advantage for the association. The building has, in consequence, been granted, and is now in the possession of the trustees. (Applause.)

The association hope that this proceeding will meet the approbation of the general committee, and that the thanks of the Association may be conveyed to the Queen, through the Earl of Lincoln, for this instance of her Majesty's gracious favour to the British Association for the Advancement of Science. (Applause.)

On the motion of the Rev. Wm. Scoresby, D.D., the report was received, and ordered to be entered on the minutes of the general committee.

R. I. Murchison, Esq., F.R.S., moved the following resolution: —"That the Earl of Lincoln be requested to convey to her Majesty the Queen, the dutiful and grateful thanks of the British Association, for her Majesty's gracious patronage and encouragement of science, in placing the Royal Observatory at Kew at the disposal of the British Association for the prosecution of scientific researches." Having been one of the deputation who waited upon her Majesty's government when the application was made, he must say that they showed extreme willingness to advance the objects of the British Association. The building was in such a condition that it would not require repairing for many years. The exact purposes to which it would be applied were not yet decided, but the subject would undergo a careful consideration in the committee of recommendations.

The Marquis of Northampton seconded the motion, observing that the granting of the building for the use of the British Association, was a proof that it was the desire of her Majesty, as well as the desire of the government, to promote the interests of science.

The resolution then passed.

On the motion of John Taylor, Esq., F.R.S., the arrangement suggested by the council as to the distribution of tickets admitting persons to the sectional meetings only, on payment of £1 each, was approved.

The following list of sectional officers and committees of sections was then adopted, with the understanding that the committees were to have the power of adding to their numbers those members of the association whose assistance they might desire:—

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE.

President: The Very Rev. G. Peacock, D.D., F.R.S., Dean of Ely.

Vice-Presidents: Sir D. Brewster, K.H., F.R.S.; Sir T. M. Brisbane, K.C.B., F.R.S.; Rev. Professor Lloyd, F.R.S.; Sir W. Hamilton.

Secretaries: Professor Stevelly, A.M.; Rev. W. Scoresby, F.R.S.; Professor M'Cullagh, M.R.I.A.

Committee: The Earl of Rosse, Professor Bessel, Professor Erman, Colonel Sabine, Rev. W. Whewell, J. Phillips, Sir J. F. W. Herschel, S. E. Cottam, W. Snow Harris, Professor Frisiani; Professor Braschmann, of Moscow; Professor Jacobi, of Königsberg.

SECTION B.—CHEMISTRY AND MINERALOGY.

President: John Dalton, D.C.L., F.R.S.

Vice-Presidents: Professor T. Graham F.R.S.; Rev. W. V. Harcourt, F.R.S.; Michael Faraday, F.R.S.; C. Henry, M.D., F.R.S.

Secretaries: Dr. Lyon Playfair, Robert Hunt, John Graham.

Committee: Wm. West, John Davies, H. C. Campbell, H. H. Watson, P. Clare, Alfred Binyon, Dr. Daubeny, H. E. Solly, Professor Nuttall, of Philadelphia.

SECTION C.—GEOLOGY AND PHYSICAL GEOGRAPHY.

President: R. I. Murchison, F.R.S., &c., Pres. G.S.

Vice-Presidents: Sir H. T. Delabeche, F.R.S., F.G.S.; Rev. W. Buckland, D.D., F.R.S., F.G.S.; Rev. A. Sedgwick, F.R.S., F.G.S.; R. Griffith, F.R.S.

Secretaries: H. E. Strickland, F.G.S.; G. Lloyd, M.D., F.G.S.; E. Binney, Sec. Manch. Geol. Soc.; R. Hutton, F.G.S.

Committee: Professor Owen, F.R.S., F.G.S.; John Phillips, F.R.S., F.G.S.; the Earl of Enniskillen, F.G.S.; M. Adolphe Erman; Count A. Von Keyserling; Dr. Dieffenbach; Mr. Schoolcraft, U.S., Hon. Mem. G.S.; J. Bateman; Dr. Black; H. Ormerod, F.G.S.; John Taylor, F.R.S., F.G.S.; W. C. Williamson; W. Gray, jun., F.G.S.; Rev. T. Egerton, F.G.S.; John Hawkshaw, F.G.S.; James Bryce, F.G.S.; H. C. Campbell; Elias Hall; Matthew Dawes, F.G.S.; Rev. D. Williams, F.G.S.; J. B. Ibbotson, F.G.S.

SECTION D.—ZOOLOGY AMD BOTANY.

President: The Hon. and Very Rev. Wm. Herbert, LL.D., F.L.S., Dean of Manchester.

Vice-Presidents: John Richardson, M.D., F.R.S.; John Moore, F.L.S.; Sir William Jardine, Bart., F.R.S.E.; The Bishop of Norwich, P.L.S.

Secretaries: Edwin Lankester, M.D., F.R.S.; Robert Paterson, F.L.S.; J. A. Turner.

Committee: J. F. Royle, M.D., G. T. Fox, H. E. Strickland, Professor Owen, Professor Henslow, John Blackwall, Captain Brown, Dr. Daubeney, John E. Gray, Richard Taylor, Sec. L.S.

SECTION E.—MEDICAL SCIENCE.

President: Edward Holme, M.D., F.L.S.

Vice-Presidents: James Lomas Bardsley, M.D.; C. B. Williams, M.D.

Secretaries: Dr. Sargent and Dr. Chaytor.

Committee: Dr. Fleming, Dr. Lyon, Dr. D. Hulme, W. J. Wilson, T. Turner, J. A. Ransome, Dr. Roget, Dr. Chaytor.

SECTION F.—STATISTICS.

President: George William Wood, M.P., F.L.S.

Vice-Presidents: Lieut. Colonel Sykes, Henry Hallam, F.R.S.; Sir Charles Lemon, Bart., F.R.S.; G. R. Porter.

Secretaries: Rev. R. Luney, M.A., G. W. Ormerod, William Cook Taylor, LL.D.

Committee: Marchese Torrigiani, Dr. Alison, His Excellency Edward Everett, John Roberton, Wm. Felkin, William Langton, P. M. James, J. Heywood, F.R.S., R. H. Greg, G. Webb Hall, Samuel Turner, J. N. Walker, H. I. Porter, F.S.S.

SECTION G.—MECHANICAL SCIENCE.

President: Rev. Professor Willis, M.A., F.R.S., &c., &c.

Vice-Presidents: William Fairbairn, C.E.; Eaton Hodgkinson, F.R.S.; Sir M. I. Brunel, F.R.S.; Sir John Robinson, Sec. R.S.E.

Secretaries: James Thompson, F.R.S., Ed.; J. Scott Russell, M.A., F.R.S., Ed.; I. F. Bateman, C.E.; C. Vignoles, F.R.A.S.

Committee: Sir George Stephenson, E. Woods, John Taylor, P. Clare, R. Roberts, J. Whitworth, J. Nasmyth, G. W. Buck, A. Liddell, Professor Moseley, J. S. Enys, J. I. Hawkins, J. Grantham, Captain Pringle, Mr. Tait, the Baron Von Bache, his Excellency M. Reichel, of St, Petersburgh, and John Kennedy.

A committee of recommendations was then nominated from among the officers of the different sections,

Professor John Phillips afterwards read the programme of the week's proceedings, printed copies of which are prepared for the use of the members. With respect to excursions, he remarked that since the sections had only five clear days of meeting, it was thought better not to make arrangements for excursions which might trench upon the sectional meetings. Arrangements, however, had been made for enabling members of the association to visit the fossil trees on the line of the Bolton Railway. There would be special trains passing from Manchester at nine o'clock on the mornings of Friday, Monday, and Wednesday, by which members would be conveyed to the fossil trees and back gratuitously, tickets having been provided for that purpose. There was also an excursion for Thursday, the 30th of June, after the meeting was over, to the Worsley tunnel and collieries, and for that purpose a boat would leave Knott Mill about eight o'clock in the morning.

The Chairman then announced that the committee would adjourn to Monday, the 27th June, at three o'clock, for the purpose of deciding the question which would then be brought forward as to the next place of meeting, and other business relating thereto.

THURSDAY.—THE SECTIONS.

The scientific business of the association, as is generally known, is transacted in several different classes or sections, which meet at the same hour, in spacious rooms provided for the purpose. The sections met for the first time on Thursday morning, at eleven o'clock, at the following places :—

Section A. (*Mathematical and Physical Science*), Royal Institution Lecture Theatre.

Section B. *(Chemistry and Mineralogy)*, Literary and Philosophical Society's Meeting Hall, George-street.

Section C. *(Geology and Physical Geography)*, Athenæum Lecture Theatre, George-street.

Section D. *(Zoology and Botany)*, Council Room of the Natural History Society's Museum, Peter-street.

Section E. *(Medical Science)*, News Room of the Mechanics' Institution, Cooper-street.

Section F. (*Statistical Science*), Refreshment Room of the Assembly Rooms, Mosley-street.

Section G. (*Mathematical Science*), Lecture Theatre of the Mechanics' Institution, Cooper-street.

Each of the sectional meeting rooms has an adjoining room, in which the respective committees of sections meet every morning at ten. The following paper occupied Section B. on Thursday :—

Dr. Playfair said that Professor Liebig had been requested, some few years ago, to apply himself to the consideration of questions in vegetable and animal physiology. The professor's first letter had been read at the meeting of the association at Glasgow, in the year 1840. The second letter he was about to bring before their notice. And in his third letter, the professor intended to apply the principles of organic chemistry to diet and dietetics ; and under this head would be comprised the nutriveness of particular vegetables in the fattening of cattle. This third report, it was at first supposed, would occupy Professor Liebig for two years ; but he (Dr. Playfair) hoped he should have the pleasure to bring it before the association at their next annual meeting. It must be borne in mind that he had prepared his abstract for a mixed and general audience like the present, and which comprised, he was happy to observe, a number of ladies. The first part of Professor Liebig's report consisted of the examination of the processes employed in the nutrition and reproduction of the various parts of the animal economy. In vegetables, as well as in animals, we recognise the existence of a force in a state of rest. It is the primary cause of gowth or increase in mass of the body, in which it resides. By the action of external influences, such as by pressure of air and moisture, its condition of static equilibrium is disturbed ; and, entering into a state of motion or activity, it occupies itself in the production of forms. This force has received the appellation of *vital force*, or *vitality*. Vitality, though residing equally in the animal and vegetable kingdoms, produces its effects by widely different instruments. Plants subsist entirely upon manures belonging to inorganic nature. Atmospheric air, the source whence they derive their nutriment, is considered to be a mineral by the most distinguished mineralogists. All substances, before they can form food for plants, must be resolved into organic matter. But animals, on the other hand, require highly-organised atoms for nutriment. They can only subsist upon parts of an organism. They possess within them a vegetative life, as plants do, by means of which they increase

in size, without consciousness on their part; but they are distinguished from vegetables, by their faculties of locomotion and sensation—faculties acting through a nervous apparatus. The true vegetative life of animals is in no way dependent upon this apparatus, for it proceeds when the means of voluntary motion and sensation are destroyed: and the most energetic volition is incapable of exerting any influence on the contractions of the heart, on the motion of the intestines, or on the processes of secretion. All parts of the animal body are produced from the fluid circulating within its organism, by virtue of vitality, which resides in every organ. A destruction of the animal body is constantly proceeding. Every motion, every manifestation of force, is the result of the transformation of the structure, or of its substance. Every conception, every mental affection, is followed by changes in the chemical nature of the secreted fluids. Every thought, every sensation, is accompanied by a change in the composition of the substance of the brain. It is to supply the waste thus produced, that food is necessary. Food is either applied in the increase of the mass of a structure (i. e. in nutrition), or it is applied in the replacement of a structure wasted (i. e. in reproduction). The primary condition for the existence of life is the reception and assimilation of food. But there is another condition equally important—the continual absorption of oxygen from the atmosphere. All vital activity results from the mutual action of the oxygen of the atmosphere, and the elements of the food. All changes in matter proceeding in the body are essentially chemical, although they are not unfrequently increased or diminished in intensity by the vital force. The influence of poisons, and remedial agents on the animal economy proves, that the chemical combinations and decompositions proceeding therein, and which manifest themselves in the phenomena of vitality, may be influenced by bodies having a well-defined chemical action. Vitality is the ruling agent by which the chemical powers are made to subserve its purposes; but the acting forces are chemical. It is from this view, and no other, that we ought to view vitality.

According to Lavoisier, an adult man takes into his system, every year, 837 lb. of oxygen, and yet he does not increase in weight. What, then, becomes of the enormous quantity of oxygen introduced in the course of the year into the human system? The carbon and hydrogen of certain parts of the body have entered into combination with the oxygen introduced through the lungs and through the skin, and have been given out in the form of carbonic acid, and the vapour of water. At every moment, with every expiration, parts of the body are thus removed, and are emitted into the atmosphere. No part of the oxygen inspired is again expired as such, Now it is found that an adult inspires 32½ oz. of oxygen daily. This will convert the carbon of 24 lb. of blood into carbonic acid. He must, therefore, take as much nutriment as will supply this daily loss; and, in fact, it is found that he does so; for the

average amount of carbon in the daily food of an adult man, taking moderate exercise, is 14 oz., which require 37 oz. of oxygen for their conversion into carbonic acid. But it is obvious, as the inspired oxygen can be removed only by its conversion into carbonic acid and water, that the amount of food necessary for the support of the animal body must be in direct ratio to the quantity of oxygen taken into the system. Thus a child, in whom the organs of respiration are naturally in a state of great activity, requires food more frequently and in greater proportions to its bulk than an adult, and is also less patient of hunger. A bird, deprived of food, dies on the third day; whilst a serpent, which inspires a mere trace of oxygen, can live without food for three months. The capacity of the chest in an animal is a constant quantity. We, therefore, inspire the same *volume* of air, whether at the pole or the equator. But the weight of the air, and consequently of the oxygen, varies with the temperature. Thus an adult man takes into the system daily 46,000 cubic inches of oxygen, which, if the temperature be 77°, weigh 32½ ounces; but, when the temperature sinks down to the freezing point (32°), it will weigh 35 ounces. Thus an adult in our climate in winter may inhale 35 ounces of oxygen; in Sicily he would inspire only 28½ ounces, and if in Sweden 36 ounces. Hence we inspire more carbon in cold weather, when the barometer is high, than we do in warm weather; and we must consume more or less carbon in our food in the same proportion. In our own climate, the difference between summer and winter, in the carpon expired, and therefore necessary for food, is as much as an eighth. Even when we consume equal weights of food, an infinitely wise Creator has so adjusted it as to meet the exigencies of climate. Thus the fruit on which the in-inhabitants of the south delight to feed contains only twelve per cent. of carbon; whilst the bacon and train oil enjoyed by the inhabitants of the Arctic regions contain from sixty-six to eighty per cent. of the same element. Now the mutual action between the elements of food and the oxygen of the air *is the source of animal heat.* All living creatures, whose existence depends on the absorption of oxygen, possess within themselves a source of heat, independent of the medium in which they exist. This heat, in Professor Liebig's opinion, is wholly due to the combustion of the carbon and hydrogen contained in the food which they consume. Animal heat exists only in those parts of the body through which arterial blood (and with it oxygen in solution) circulates. The carbon and hydrogen of food, in being converted by oxygen into carbonic acid and water, must give out as much heat as if they were burned in the open air. The only difference is, that this heat is spread over unequal spaces of time; but the actual amount is always the same. The temperature of the human body is the same in the torrid as in the frigid zone. But, as the body may be considered in the light of a heated vessel, which cools with an accelerated rapidity the colder the surrounding medium, it is obvious that the fuel

necessary to retain its heat must vary in different climates. Thus less heat is necessary in Palermo, where the temperature of the air is that of the human body, than in the Polar regions, where it is about 90 degrees lower. In the animal body the food is the fuel, and by a proper supply of oxygen we obtain the food given out during its combustion in winter. When we take exercise in a cold atmosphere, we respire a greater amount of oxygen, which implies a more abundant supply of carbon in the food; and by taking this food we form the most efficient protection against the cold. A starving man is soon frozen to death; and every one knows, that the animals of prey of the Arctic regions are far more voracious than those of the torrid zone. Our clothing is merely an equivalent for food; and the more warmly we are clothed, the less food we require. Were we to go destitute of clothes, like certain savage tribes,—or if, in hunting or fishing, we were exposed to the same degree of cold as the Somoyides,—we could with ease consume 10lb. of flesh, and perhaps a dozen tallow candles into the bargain, as warmly clad travellers have related, with astonishment, of those people. Then could we take the same quantity of brandy or blubber of fish, without bad effects, and learn to appreciate the delicacy of train oil. We thus perceive an explanation of the apparently anomalous habits of different nations. The maccaroni of the Italian, and the train oil of the Greenlander and the Russian, are not adventitious freaks of taste, but necessary articles fitted to administer to their comfort in the climates in which they have been born. The colder the region, the more combustible must the food be. The Englishman in Jamaica perceives with regret the disappearance of his appetite, which, in England, had been a constant recurring source of enjoyment. By the use of aromatics, he creates an artificial appetite, and eats as much food as he did at home. But he thus unfits himself for the climate in which he is placed; for sufficient oxygen does not enter his system to combine with the carbon consumed; and the heat of the climate prevents him taking exercise to increase the number of his respirations. The carbon of the food is therefore forced into other channels, and disease results. England, on the other hand, sends her dyspeptic patients to southern climates. In our own land, their impaired digestive organs are unable to fit the food for that state in which it best unites with the oxygen of the air, which therefore acts on the organs of respiration themselves, thus producing pulmonary complaints. But when they are removed to warmer climates, they absorb less oxygen, and take less food; and the diseased organs of digestion have sufficient power to place the diminished amount of food in equilibrium with the respired oxygen. Just as we would expect from these views, in our own climate, hepatic diseases, or diseases arising from excess of carbon, are more prevalent in summer; and in winter pulmonic diseases, or those arising from excess of oxygen. The professor then went on to disprove the notion, that animal heat is due to nervous influence, and

not to combustion—an error which had its origin in supposing that the combustion proceeds in the blood itself. He also showed that animal heat must not be ascribed to the contraction of the muscles. The professor proceeded to prove, that the heat evolved by the combustion of carbon in the body is sufficient to account for the phenomena of animal heat. He showed that the 14 ounces of carbon which are daily converted into carbonic acid, in an adult, disengage no less than 197·477° of heat; a quantity which would convert 24 lb. of water at the temperature of the body, into vapour. And if we assume that the quantity of water vaporised through the skin and lungs amounts to 3 lb., then we have still 146·380° of heat to sustain the temperature of the body. And when we take into calculation the heat evolved by the hydrogen of the food, and the small specific heat possessed by the organs generally, no doubt could be entertained that the heat evolved in the process of combustion, to which the food is subjected in the body, is amply sufficient to explain the constant temperature of the body. From what has preceded, it is obvious that the amount of carbon consumed in food ought to depend on the climate, density of air, and occupation of the individual. A man will require less carbon when pursuing a sedentary occupation than when he is engaged in active exercise. Professor Liebig, having thus discussed the source of animal heat, proceeds next to consider what are the ingredients in the food, which may properly be considered to be nutritious. Physiologists conceive that the various organs in the body have originally been formed from blood. If this be admitted, it is obvious that those subjects only can be considered as nutritious which are susceptible of being transformed into blood.—The professor then entered upon an examination of the composition of blood, and of the identity in chemical composition of fibrine and albumen. The nutritive process is simplest in the case of the carnivora. This class of animals live on the blood and flesh of the graminivora, whose blood and flesh is identical with their own. In a chemical sense, therefore, a carnivorous animal, in taking food, feeds upon itself, for the nutriment is identical in composition with its own tissues. The professor then inquired from what constituents of vegetables the blood of the graminivorous is produced. The nitrogenized compounds of vegetables forming the food of graminivorous animals are called vegetable fibrine, vegetable albumen, and vegetable casein. Now, analysis has led to the interesting result, that they are exactly of the same composition in 100 parts; and, what is still more extraordinary, *they are absolutely identical with the chief constituents of the blood;* animal fibrine and animal albumen. By identity, be it remembered, we do not imply similarity, but absolute identity, even as far as their inorganic constituents are concerned. These considerations showed the beautiful simplicity of nutrition. In point of fact, *vegetables* produce, in their inorganism, the blood of *all animals.* Animal and vegetable life are therefore most closely connected. The professor has still to account for the

use of the substances in food which are absolutely destitute of nitrogen, but which we know are absolutely necessary to animal life. In all these we find a great excess of carbon, and but very little oxygen. By a train of admirable reasoning, the professor arrives at the interesting conclusion, that they are solely exhausted in the production of animal heat, being converted by the oxygen of the air into carbonic acid and water. This portion of the report contained an ingenious and important view of the use of bile in the animal economy, the truth of which quantitative physiology dare not deny. When exercise is denied to graminivorous and omnivorous animals, this is tantamount to a deficient supply of oxygen. The carbon of the food not meeting with sufficient oxygen to consume it, it passes into the compounds containing a large excess of carbon, and deficiency of oxygen; or, in other words, *fat* is produced. Liebig is led to the startling conclusion that fat is altogether an abnormal and unnatural production, arising from the adaptation of nature to circumstances, and not of circumstances to nature: altogether arising from a disproportion of carbon in the food to that of the oxygen respired by the lungs, or absorbed by the skin. Wild animals in a state of nature do not contain fat. The Bedouin, or Arab of the desert, who shows with pride his lean, muscular, sinewy limbs, is altogether free from fat: and the professor points out the diseases arising from this cause, and furnishes some valuable hints to therapeutics. From all that has transpired, we may sum up the nutritious elements of food as follow. The ingredients adapted for the formation of the blood, and which the professor calls the *plastic elements of nutrition*, are as follow:—Vegetable fibrine, vegetable albumen, vegetable caseine, animal flesh, animal blood. The other ingredients of food being fitted to retain the temperature of the body, he calls the elements of respiration. They are—fat, starch, gum, cane sugar, grape sugar, sugar of milk, pectine, bassorine, beer, wine, spirits. These are Professor Liebig's general principles of nutrition. The second part of the work consists of details, in which he examines the chemical processes engaged in the production of bile, of urea, uric acid and its compounds, as well as of cerebral and nervous substance. The conclusions to which he has arrived on these subjects are of such great and startling interest, that Dr. Playfair said, he dare not venture to make an abstract of them, without entering into the calculations with which they are accompanied. In the professor's explanatory remarks on digestion, he ascribes a singular function to saliva. This fluid possesses the remarkable property of enclosing air in the shape of froth, in a far higher degree even than soap suds. This air, by means of the saliva, accompanies the food into the stomach, and there its oxygen enters into combination with the constituents of the food, whilst its nitrogen is again given out through the lungs or skin. The longer digestion continues, the greater is the quantity of saliva, and consequently of air, which enters the stomach. Rumination, in certain graminivorons animals, has plainly for one object a renewed

and repeated introduction of oxygen. The professor further touches upon the use of tea and coffee as an article of food. Recent chemical research has proved, that the active principles of tea and coffee—viz. tein and caffein—are absolutely one and the same body, perfectly identical in every respect. The action of tea and coffee on the system must be therefore the same. How is it that the practice of taking them has become necessary to whole nations? Caffeine (theine) is a highly nitrogenised body. Bile, as is well known, contains an essential nitrogenised ingredient—*taurine*. Now, Professor Liebig considers, that caffeine goes to the production of this taurine; and, if an infusion of tea contains only one-tenth of a grain of caffeine, still if it contribute, in point of fact, to the formation of bile, the action even of such a quantity cannot be looked upon as a nullity. Neither can it be denied that, in case of using an excess of non-azotised food, or deficiency of motion, which is required to cause the change of matter in the tissues, and thus to yield nitrogenised matter of the bile, that in such a condition the state of health may be benefited by the use of tea or coffee, by which may be furnished the nitrogenised product produced in the healthy state of the body, and essential to the production of an important element of respiration. The American Indian, with his present habits of living solely on flesh, could not with any comfort use tea as an article of food; for his tissues waste with such rapidity that, on the contrary, he has to take something to retard this waste. And it is worthy of remark, that he has discovered in tobacco smoke a means of retarding the change of matter in the tissues of his body, and thereby of making hunger more endurable. Nor can he withstand the captivation of brandy, which, acting as an element of respiration, puts a stop to the change of matter, by performing the function which properly belongs to the products of the metamorphosed tissues. The third part of Professor Liebig's report treats of the recondite laws of the phenomena of motion. As it is principally of a speculative character, we can pass this over. The professor concludes his valuable communication by two chapters: one on the theory of disease; the other on the theory of respiration. The whole life of animals consists of a conflict between chemical forces and the vital powers. In the normal state of the body of an adult, both stand in equilibrium. Every mechanical or chemical agency which disturbs the restoration of this equilibrium is a cause of disease. Disease occurs when the resistance offered by the vital force is weaker than the acting cause of disturbance. *Death* is that condition in which chemical or mechanical powers gain the ascendancy, and all resistance on the part of the vital force ceases. Every abnormal condition of supply or waste may be called disease. It is evident that one and the same cause of disease—that is, of disturbance—will have different effects, according to the period of life. A cause of disease, added to the cause of waste, may in old age annihilate the resistance of the vital powers, or, in other words, occasion death; while, in the adult state,

it may produce only a disproportion between supply and waste; and in infancy only an abstract state of health, *i.e.*, an equilibrium between supply and waste. Professor Liebig argues, from what has preceded, that a deficiency of resistance in a living part to the cause of waste is in fact a deficiency of resistance to the action of the oxygen of the atmosphere. The professor's theory may be compared to a self-regulating steam-engine. The body, in regard to the production of heat and of force, acts just like one of those machines. With the lowering of the external temperature, the respiration becomes deeper and more frequent; oxygen is supplied in greater quantity, and of greater density; the change of matter is increased, and more food must be supplied, if the temperature of the body is to remain unchanged. It has been proved, that *iron* is *not* necessary to the colouring matter of the blood, but that it forms an essential constituent of blood globules. These globules, it is well known, take no part in nutrition. Professor Liebig conceives, that the iron is the great means of conveying to the lungs the carbonic acid formed in the system; and he has made a calculation, that the iron contained in the body could actually convey twice as much carbonic acid as is expelled daily from the system.—Dr. Playfair concluded by warmly disclaiming that his abstract had done justice to the report, and recommending his hearers to consult the report itself. In the opinion of all, Liebig may be considered a benefactor to his species for the interesting discoveries in agriculture published by him in the first part of this report; and having in that pointed out means by which the food of the human race may be increased; in the work now before us, he follows up the chain to its continuation, and shows how that food may best be adapted to the nutrition of man. Surely there are no two subjects more fitted than these for the contemplation of the philosopher; and, by the consummate sagacity with which Liebig has applied to their elucidation the powers of his mind, we are compelled to admit that there is no living philosopher to whom the section could more appropriately have entrusted their investigations. (Much applause.)

Mr. Clare moved the thanks of the meeting to Professor Liebig for his valuable report.

The Rev. Vernon Harcourt seconded the motion. He thought it most honourable to the association, that Professor Liebig was thus connected with him in his researches. The meeting was also greatly indebted to Dr. Playfair for his very luminous and able abstract of the report.

Dr. Playfair said he had been applied to by two sections,—the medical section and the natural history section,—for the paper; but, as he had written it in a popular form for a mixed audience, it was hardly adapted for the former professional body.

Mr. Solly then read a paper, communicated by Professor Schoenbein, upon the "Electrolising Power of a simple Voltaic Circle," in which he showed the influence of hydrogen, oxygen, chloride, and other chemical bodies, when not in contact with the poles.

Mr. Wm. Blyth then read an interesting paper upon the "Manufacture of Sulphuric Acid," the different processes of which he illustrated by decomposing the crystalline deposit in the vitriol chamber in acid of the respective specific gravities of 1·400, 1·450, and 1·500. From experiments he made with indigo and this deposit, he found that it had the same destructive properties as nitric acid. In an interesting discussion upon the paper, relative to the mixture of water with sulphuric acids, by which the anomalous case of the miscibility but non-solubility of sulpuric acid in water was maintained: Mr. Davies stated, as an analogous case, that of nitric acid, when at the specific of 1·42, could not be reduced either by distillation or boiling, but at a higher state was easily reducible, or at a lower to be raised to that point; which, in his opinion, seemed to establish the point of the 1·42 to be the fixed point of those acids.

The next paper upon the list to be read was that of Mr. John Mercer, upon some "Peculiar Instances of Chemical, or (so called) Catalytic Action," but owing to the indisposition of the gentleman who should have brought it forward, it was withdrawn for presentation at the sitting of the section the next day.

Professor Haidinger had communicated an "Account of the Mineralogical and Geological Museum of the Imperial Mining Department of Vienna," but owing to its extreme length, and the somewhat uninteresting nature of its contents, it was ordered that the secretary prepare an abstract of the account, for a future meeting of the section.

Owing to the two papers mentioned above not having been read to the section, the meeting separated about one o'clock; an hour considerably earlier than usual. There had been a very fair attendance, including several ladies.

SECTION C.—GEOLOGY AND PHYSICAL GEOGRAPHY.

Mr. Strickland, the secretary, read a paper "On the Physical Structure of the Appalachian Chain," by Professor Rogers, of America. The paper gave a particular description of the geological construction of the chain, from which the author deduces general laws, which he conceives apply to all mountains in different parts of the world, wherever they have been examined. The paper referred to the dips of the rocks in the chain of mountains referred to, and by comparing their structure with that found in European chains, the author deduces the fact that there is a general undulation of the strata towards the north-west. He supposes the elevation and dips of the chains to be caused by the force of subterranean waves, and arrives at the conclusion that no other hypothesis has been found to account for the structure of the Appalachian chain. This supposed wave-like motion beneath the crust of the earth, he attributes to the agency of earthquakes, and shows that at periods subsequent to the formation of the Appalachian chain similar effects have been produced.—Thanks were voted to Professor H. B. Rogers and Wm.

B. Rogers, of the United States, for the communication on this subject, after which a discussion took place. Sir Henry De la Beche remarked that the authors of the paper imagined a quantity of molten matter beneath the crust of the earth, and that by frequent pulsations the superincumbent matter was squeezed into the shape in which it was found. He (Sir H. De la Beche) did not see that it was necessary, in order to account for those geological phenomena, to imagine the existence of this molten matter. The old theory was that in cooling the molten matter beneath the crust of the earth diminished in heat in a greater ratio than the crust, and that on that account the crust of the earth was broken up into large masses. That was the generally received theory upon which we sought to account for the formation of the earth, and its breaking up into a variety of forms. This breaking up, however, was not common in all parts of the world, for in Russia there was found a very even bend. The date of the Appalachian chain corresponded with the date of the rocks in Wales, and here was a somewhat remarkable coincidence, for in Wales there was the same breaking up as was described in the paper in reference to the Appalachian chain.—Dr. Buckland followed, and paid a high compliment to the two American geologists who had contributed the paper.—Professor Sedgwick agreed with the general facts of the authors, but could not give his accordance to their theory. The facts, he contended, were against the theory, and that what was known to exist in regard to the formation of rocks in this country did not at all bear out the conclusions to which the authors had arrived. The generalization of the authors, he said, was far too broad; they had not taken into sufficient consideration the formation of rocks in other countries; and, lastly, they had come to conclusions in the very teeth of facts. He then stated his objections to the theory advanced, and took occasion to caution the geologists present how they departed from facts. If it were true that mountain elevations had been formed in any way, he was of opinion that the granite rocks had been hoisted up, in the first instance, through fissures or cracks caused by the earth's cooling, and that then, through new powers coming into existence, from the cooling of the granite mass, or any other cause, new elevations arose in the outskirts. Although he did not agree with the theory of the authors of the paper, he still admitted that the facts they had collected were valuable.

THE ANNALS

OF

ELECTRICITY, MAGNETISM,

AND

CHEMISTRY;

AND

GUARDIAN OF EXPERIMENTAL SCIENCE.

AUGUST, 1842.

On the Electric Column. *By* J. A. De Luc, Esq., F.R.S.

PART II.

On the Electric Column, as Aerial Electroscope.

(Continued from page 14*).*

I have said in the preceding paper that, the sticking of the gold leaves to the side of the electroscope being an obstacle to regular observations of their strikings, which, however, I considered as the most proper manner of observing the variations of this instrument, I had intended to increase its power, so far as to produce the motion of a small metallic ball, in hopes that the latter would not stick.

I began this attempt by uniting together in one the three columns of 200 groups mentioned in the first part, which I had before used by only connecting them with one another; and I thus formed the column of 600 groups, making a part of the instrument represented by the figure annexed to the first part: see Pl. I. With this column I made the first trial of procuring the motion of such a small pendulum in the following manner. I connected with each extremity of the column, a wire terminated by a small brass ball; and each wire being moveable, I could bring the two balls in front of the instrument, within the distance of each other that might be found convenient; and I suspended by a silk thread a small gold bead, which I could easily bring between the balls, either at the middle distance, or nearer one than the other

The apparatus being thus prepared, I tried for a long time to make it answer my purpose, but without success. When the two balls were near each other, the bead struck them alternately with such a rapidity, that it was impossible to count the number of oscil-

lations in a determined time: a necessary condition for my purpose. I increased the distance of the balls, but I was disappointed in a different manner: when the bead was equally attracted on both sides, it sometimes remained motionless for hours in the middle of the interval; and when the attraction became stronger on one side, the bead drawn that way struck one of the balls, some oscillations then began, still too rapid and very irregular, but at the end of a little time they ceased again, the bead remained motionless at the middle point. I tried various distances of the balls, and also different degrees of approximation of the bead to either of them: sometimes there was an appearance of success, but at last the bead was again at rest in the middle space, or it stuck to one of the balls. This want of success persuaded me that a neutral pendulum, such as was the bead suspended by a silk thread, could never answer the purpose of the regular strikings, which were necessary; that the bead was to be connected by a metallic wire, with one of the extremities of the column, near a ball united with the latter, and to strike against another ball connected either with the other extremity of the column, or with the ground; the latter of which modes I first adopted.

But before I proceed, I must mention, that while I was employed in these trials at Windsor, Mr. B. M. Forster had succeeded at Walthamstow, near London, with a power of 1,500 groups of the same diameter as mine, divided into three chaplets, as described in my first paper presented to the Royal Society, to set in motion a brass ball, suspended by a silk thread between two small insulated bells, connected with the two extremities of this long column. Mr. Forster has described this new kind of chime in a letter to Mr. Tillock, published in the *Phil. Mag.* of the latter, mentioning this singular circumstance, that the ringing began on the jubilee day. Since that time I have had the pleasure of being personally acquainted with Mr. Forster, and he has lately informed me, that, having mounted again this chime on the 25th of March, it has not ceased to ring ever since. This is a curious application of the property of the column, but, as I have explained, it cannot answer the purpose of regular observations.

For the execution of the above mentioned plan, I first made the following addition to my apparatus. At the top of one of the pillars of the column, on its side A (the positive extremity), I fixed a brass piece 13, held there by a ferrule, and projecting forwards about 1½ inch: on this projection is fixed, by a screw, another brass piece, having on one side a vertical groove 14, in which is held, by a pin, a brass rod, at the lower part of which is a large brass ball 15, which can be moved backwards and forwards, in order to bring it to the convenient point, where it remains steady, by the friction of the top of the rod in the groove. From this top projects a brass loop 16, to which is suspended a gold bead 17, by the thinnest silver wire, such as is used for cross wires in telescopes; and by moving pro-

perly the ball 15, the bead is made to hang close to it, without leaning against it; this is done while both the bead and the ball are neutral, by handling the lattter for this adjustment.

My purpose having only been at first, that the ball 18, should be in communication with the ground, I produced this communication in a simpler manner than is represented in the figure, having changed it since for a purpose that I shall explain, but the difference is here of no consequence: it was then only held at the top of a brass stem, fixed to the end of a thick slip of lead 19, 19: moveable backward and forward between pins 20, 20, 20, in order that the distance of this ball from the ball 15 might be changed, as should be found proper for the strikings of the bead.

This apparatus was finished in the beginning of last spring; but it was too late for the most important observations; especially as the apparatus itself was far from being settled. Before that season, the effect of the column had been so great, that the gold leaves of the electroscopes, either on one side or the other, struck sometimes in the afternoon sixty times in a minute, even in glass tubes of one and a half inch diameter; but now they seldom struck more than once in a minute, sticking always as usual (which was the reason why I had given up the gold leaves): however, the effect was still sufficient to try the new apparatus.

This again gave me much trouble: for though at first it appeared to answer my purpose, as the gold bead receding from the ball 15, struck the ball 18, fell and returned again, with the usual changes in the frequency which were to be the object of observation, the bead at last stuck to the ball 18. I tried whether by increasing the distance of the latter, the bead thus drawn further out of the vertical line, would have more power to resist the cause of its sticking; which it is difficult to understand, as it is not the case when set in the same motion by a mechanical impulse. The strikings were less frequent; not so however as to prevent the observation, if the sticking had been prevented; but it again took place, and even more easily, as the bead arrived more slowly in contact with the ball. I did not succeed better by increasing the power (in a manner which I shall explain hereafter), though I could produce the strikings at a greater distance; so that after much labour, I had some time despaired of success, when another idea occurred to me, which however did not succeed at first.

The general idea was to produce the strikings, not by the bead itself, but by its silver wire, in a part at some height above the latter, by the wire meeting there the edge of a horizontal brass lamina in communication with the ground; in order that the wire being bent at that point, by the bead moving still farther, the latter should have a greater angular motion from that point, by a shorter radius, and have thus a greater tendency to fall back after being discharged; suppressing at the same time the ball 18, to which it stuck; but by this suppression, the motion was so much diminished,

that I was obliged to use again the ball, and then the wire itself stuck to the edge of the lamina. Following, however, farther the idea of discharging the bead by its wire meeting with the smallest conducting mass possible, I thought of substituting for the brass lamina a single silver wire like that of the bead; and at last I thus succeeded. This is the part of the figure which I am going to describe.

The last alteration I found necessary to make in the apparatus, which is represented in the figure, not being made at that time, it must be supposed, for the present, that the piece 28 is represented by the lead slip 19, 19. The piece 23 is a brass spring of about half an inch in breadth, at the base 24, where passing under the bent part of an upright brass piece 25, it is thus fixed with the latter, by screws, on the base. The breadth of the spring 23 diminishes toward its end, where it is terminated by a brass wire bow 22; in this is stretched the thin silver wire 21, against which that of the bead comes to strike. The upright brass piece 25 has at the top a screw 26, pressing against the spring, and serving to produce small motions, backwards or forwards, of the horizontal wire 21, previously brought nearly to the proper distance, by moving the lead base 19, 19. The moment of the meeting of the two silver wires is to be an instant before the bead strikes the ball 18: then, by a jerk produced at the meeting of the wires, the sticking of the bead to the ball is effectually prevented.

This was only finished in the beginning of last April; the strikings of the bead were then regular and uninterrupted, while there was no shake of the apparatus itself; but being on a table, I soon found, that by walking in the room, and also by the agitation of the air in opening and shutting the door, the motions of the bead were disturbed. This determined me to fix, against the side of the room which had a proper light, a glazed box, in which I placed the apparatus; and I fixed under it, at its level, a little table, in order to place there additional columns, which became necessary to increase the power of the instrument.

This apparatus being at last ready for regular observations, I began the meteorological journal which I had in view, including, with the number of the strikings of the bead in a certain time and the observation of the barometer, the degrees of the thermometer and of my hygrometer in the room, the only place of which it could be supposed that the changes of the temperature and of the degree of moisture might affect the state of the column: I shall copy here the journal of these observations during the few days in which the fundamental column of 600 groups still acted alone, after which time I was obliged to increase the number of the groups.

April	H.		Barom.	Thermom.	Hygrom.	Num. of strik. in 10 min.
8.	8	A.M	29·7	57	41	11
	11		id.	60	id.	10
	1	P.M	id.	60½	id.	12
	3		id.	63	id.	20
	5		id.	62½	id.	10
	7		id.	61	id.	8
	11		id.	61	id.	7
9.	8	A.M	id.	58	42	11
	11		id.	60½	id.	9
	11	P.M	id.	63	41	3
10.	11	A.M	29·8	61	id.	6
	2	P.M	id.	60	id.	5
	11		29·9	58	id.	2
11.	7	A.M	id.	50	id.	3
	4	P.M	30	56	id.	2
12.			30·1	No striking the whole day.		

This cessation of striking having lasted two days more, I judged that we were entering into the season when, in the two years before, the strikings of the gold leaves themselves had also ceased; so that in order to carry on the observations as long as possible towards the summer, it was necessary to increase the number of the groups This I undertook without changing the situation of the fundamental instrument, which, on account of the necessary steadiness and of being sheltered from currents of air, was to remain in the glazed box fixed against the side of the room. I made use therefore of the little table above mentioned for placing on it additional columns, which I made upright like the common pile, as more easily managed; and knowing that with the same number of groups, the strikings would be accelerated by larger plates, but that it was a long and tedious operation to cut them round, I determined to make a column with square plates. For this purpose I bespoke some sheets of laminated zinc about the thickness of a card; but those which I received were so much puckered that I despaired of their being fit for my purpose; however I obtained flat pieces of them by a method which it may be useful to explain.

I procured a good pair of hand-shears, and with these I first cut the puckered sheets into slips of one and a quarter inch breadth, as nearly as they could be traced upon such an uneven surface; and placing many of these slips upon one another between two pieces of hard wood, I pressed them with force in a vice, leaving them there for half an hour: they came out very flat, only not very straight, but this could be mended. Zinc, in this malleable state having nearly the softness of lead, stretches laterally by that great pressure, and thus the puckerings are effaced. Making then straight with a file one side of the slips, I marked on this edge with a divider,

points at one and a quarter inch distance ; and by these points I traced with a square the plates to be cut with the shears : these pieces were distorted by the cutting ; but placing them also over one another, by scores, between two thick plates of brass, and pressing them strongly in the vice, they became flat ; and I had only to round a little the angles with a file, placing them again in the vice without the brass plate.

In this manner I made 300 zinc plates one inch and a quarter square, and having cut an equal number of pieces of Dutch-gilt paper of the same size, I mounted this upright column between four glass rods covered with sealing wax, fixed in a wooden base. This column, loose between the rods, is supported at the bottom on four insulating pillars one inch and a half high, on which is first laid a brass plate with a projecting part of about two inches, at the extremity of which is a large hole for receiving the end of proper conductors ; and for the same purpose the column is terminated at the top by a similar plate, on which presses a screw in the common manner used for the pile. The top of this column, which is its positive extremity, was to be connected with the negative extremity of the horizontal column, and this required that the box containing the latter should be opened in front, I therefore placed only a pane of glass on the side where the bead hangs, in order to guard it against the motions of the air.

The first use which I made of this additional column with larger plates, was for the following experiments.

Experiment 16. Placing the negative (or lower) extremity of the column composed of the large square plates, in communication with the ground, I connected its positive extremity with a gold leaf electroscope, and after having observing the maximum, soon produced, of its divergence, I substituted for this column 300 of the small groups of the horizontal column, by placing the communication with the ground at its middle point : this produced the same divergence as the former, but it required more time.

I could not compare directly the effects of the two columns, with respect to the frequency of strikings of the bead, because at that time 300 groups of any size were no longer sufficient for producing them ; but I compared the effects of the two columns for this purpose in the following manner.

Exp. 17. I first repeated the observation of the strikings with the horizontal column of 600 groups, its negative extremity being in communication with the ground : there were three strikings in five minutes. I then took off the communication of this column with the ground, and connecting its middle point with the positive extremity of the column of 300 groups of the square plates, I placed the negative extremity of the latter in communication with the ground. This was again 600 groups, but 300 of them were of larger plates, and there were then seven strikings in the same time ; and thus was confirmed what I had judged of the effect of larger plates for increasing the frequency of the strikings.

Exp. 18. I now connected the new column of 300 larger plates with the 600 groups of the horizontal column, leaving the communication with the ground at the negative extremity of the former. This was in a more favourable moment; for the addition of 300 of the small groups ought to have produced with the whole only eight and a half strikings in five minutes, and there were ten.

From this increase of power I expected a greater duration of observations in this season, but I soon saw a diminution in the frequency of the strikings, which disappointed me. I lost again much time in changing the arrangement of the apparatus, in order to take in also the two columns formed of tinned iron plates, mentioned in my first paper: there were 700 of these groups, which produced in the gold leaves nearly the same divergence as the column of 300 zinc plates one and a quarter inch square. This being the only change that I could undertake for the season, I began another course of observations; and it will be seen in the following journal how rapidly the effect went on diminishing.

May	H.	Barom.	Thermom.	Hygrom.	Numb. of strik. in 5 min.
10.	8 A.M.	30.15	63	40	14
	12	id.	67	id.	19
	2 P.M.	30·17	67	id.	17
	4	30·20	66	id.	16
	8	30·25	65	id.	12
	10	id.	65	id.	10
11.	7 A.M.	id.	59	41	7
	11	id.	64	39½	8
	2 P.M.	id.	67	id.	11
	4	id.	67	id.	9
	10	id.	64	39	4
12.	7 A.M.	30·19	57	39½	4
	11	30·18	60	39	4
	3 P.M.	30·15	65½	38¾	6
	10	30·10	65½	39	4½
13.	9 A.M.	30·05	63	40	7½
	1 P.M.	30·02	65	39½	7
	10	30	64	39¼	2
14.	9 A.M.	29·85	62	39½	5
	12	29·80	65	id.	7
	3 P.M.	id.	67	id.	8
	11	29·75	66	39	no striking.
15.	7 A.M.	29·65	61	40	
		No striking the whole day.			
16.	7 A.M.	29·6	64	40½	½ or 1 in 10 min.
	11	29·65	63	39½	5
		No striking in the evening.			

May	H.		Barom.	Thermom.	Hygrom.	Numb. of strik. in 5 min.
17.	7	A.M	29·83	59	40	1
	10		id.	63	39¾	3½
	11	P.M	29·65	66	id.	5
18.	7	A.M	29·75	59	40	3¾
	12		29·80	63	39¼	3¾
	11	P.M	29·95	61½	39	2
19.	7	A.M	30·1	56	39½	no striking.
	4	P.M	30·2	65	id.	4
			No striking in the evening			
20.	8	A.M	30·18	57	39½	2½
	4	P.M	30·08	67½	39	4
	10		30	67	38¾	2
21.	7	A.M	29·9	60	40	½
	2	P.M	29·83	66	39¾	5¼
	11		29·9	65	40	3
22.	7	A.M	30·1	60	40½	2½
	5	P.M	30·15	69	40	5½
	10		30·2	64	40½	4¼
23.	8	A.M	30·3	60	40½	3¼
	12		id.	63	39½	4
			No striking for a long time.			
	7½	P.M	id.	66	39	2
24.	6½	A.M	30·38	59	39¼	1¾
	2	P.M	30·32	66	39	4
	10½		30·3	65	id.	1½
25.	6½	A.M	30·25	61	39¾	2½
	3½	P.M	30·24	69	38½	3
	10½		30·28	68	39	2
26.	7	A.M	30·23	59	40	2½

This great diminution in the frequency of the strikings made me think of connecting the ball 18 with the negative extremity of all the columns, in order to see what increase it would make in the frequency: but luckily at the same time it occurred to me, that, by producing a speedy manner of changing this connexion of the ball for that with the ground, and inversely, it would be a mode of discovering variations in the electric state of the latter, by comparing, in a short time, its effect on the strikings, with that of the negative extremity of the columns. This was the occasion of the last change which I made in the apparatus, as represented in the figure, which part I am now going to describe.

On the lead base 19, 19, I fixed two insulating pillars 27, 27, and on these a brass piece 28, at one extremity of which is fixed the ball 18, and at the other the machinery for moving the horizontal silver wire 21. By this insulation of the parts against which the bead and its silver wire come to strike, I can place them in a moment in

communication with either the ground or the negative extremity of the column, by only changing the position of a brass wire 29, hooked to the extremity of the horizontal brass piece. In the position of this wire, as represented in the figure, the ball 18 and the silver wire 21 are in communication with the ground; and when it is wanted to make them communicate with the negative extremity of the columns I have only to take up the moveable wire, and to lay its end on the projecting brass piece of that extremity. I was surprised to find so little difference of effect between the communinication of the ball 18 with the negative extremity of the columns, and with the ground, which is a standard between the negative and positive state of bodies; and upon the whole this kind of observation opens a new and interesting field of researches. Therefore, though I had but a short time to follow these observations, the following journal will show at least the nature of this phenomenon.

						No. of strikes in 5 min.	
			Barom.	Thermom.	Hygrom.	With the ground.	With the negat. extrem. of the col.
May	H.						
26.	4	P.M..	30·15	71	$38\frac{3}{4}$	3	6
	9		id.	$68\frac{1}{2}$	39	2	2
27.	8	A.M..	30·13	61	$39\frac{1}{2}$	2	2
	11		30·11	$62\frac{1}{2}$	$39\frac{1}{4}$	2	3
28.	1	P.M..	30·30	65	$38\frac{1}{2}$	1	2
	3		30·40	66	id.	2	$2\frac{1}{4}$
29.	7	A.M..	30·55	56	id.	$\frac{1}{2}$	1
	12		id.	64	$37\frac{3}{4}$	$1\frac{5}{8}$	$2\frac{1}{4}$
	8	P.M..	30·53	68	$37\frac{1}{2}$	$\frac{7}{8}$	3
30.	7	A.M..	id.	$58\frac{1}{2}$	$38\frac{1}{2}$	$\frac{5}{8}$	$1\frac{1}{5}$
	$1\frac{1}{2}$	P.M..	30·50	66	38	$4\frac{1}{2}$	4
	4		id.	$69\frac{1}{2}$	$38\frac{1}{2}$	4	$3\frac{1}{4}$
	9		id.	67	$38\frac{1}{4}$	$1\frac{1}{4}$	2
31.	7	A.M..	id.	60	$38\frac{1}{2}$	1	$1\frac{2}{5}$
	2	P.M..	30·45	68	id.	4	3
June	$9\frac{1}{2}$		30·48	69	id.	2	$1\frac{1}{2}$
1.	8	A.M..	30·45	63	$39\frac{1}{2}$	4	4
	11		30·44	64	$39\frac{1}{4}$	4	$4\frac{1}{4}$
	3	P.M..	30·45	69	$37\frac{1}{2}$	$1\frac{2}{3}$	$1\frac{2}{3}$
	$9\frac{1}{2}$		id.	69	38	$1\frac{1}{4}$	$1\frac{2}{5}$
2.	7	A.M..	30·42	62	39	$1\frac{2}{3}$	2
	11		id.	65	$38\frac{3}{4}$	$2\frac{1}{2}$	3
	2	P.M..	30·38	$69\frac{1}{2}$	$38\frac{1}{4}$	4	4
3.	$6\frac{1}{2}$	A.M..	id.	64	$38\frac{1}{2}$	$\frac{5}{8}$	$1\frac{1}{4}$
	10		30·40	65	id.	2	2
	$4\frac{1}{2}$	P.M..	30·38	69	$37\frac{3}{4}$	$1\frac{2}{3}$	$1\frac{2}{3}$
4.	8	A.M..	30·45	$60\frac{1}{2}$	$38\frac{1}{2}$	$1\frac{3}{7}$	$1\frac{2}{3}$
	11		30·40	63	id.	$1\frac{7}{8}$	$2\frac{1}{7}$
	$2\frac{1}{2}$	P.M..	id.	68	38	$2\frac{1}{2}$	$2\frac{1}{2}$

In the last days of these observations I had some reason to suspect that something had been deranged in the apparatus, but I could not examine it, as I was preparing to leave Windsor for spending the summer in Devonshire, where I write this paper. However, the defect which I suspected did not interfere with the object of this last series of observations, which principally relates to the electric state of the ground. This state is here compared with that of the extremities of the column, which I have called negative, though it is sometimes neutral, comparatively with the electric state of the ambient air; but it is never positive. On the other hand, the bead never moves but as positive comparatively with the same standard, and it moves the faster as the ball 18 differs more from its electric state. Now it is seen in the above observations, that sometimes the bead moves faster when the ball is in communication with the ground, than when it communicates with the extremity of the column called negative. This is a test of the electric state of the ground which deserves to be deeply studied, in order to understand it better.

Were I younger, I ought not to publish these experiments and observations in their present state; I should endeavour first, to improve the instrument, in order to meet a proper season with more advantage; then to follow the motion of the aerial electroscope more regularly than hitherto I have been able to do, being constantly employed in improving it; and to study the connexion between these motions and the changes in the electric state of the air near the ground, and of the ground itself: a course of observations, which is to be followed from the time of the greatest effects of the column, to that of their rapid diminution, coinciding with the time when vegetation, the greatest terrestrial phenomenon, prevails on all the ground; and in which it thus appears that the electric fluid has some influence. But though it is possible that I may take up again these observations, I prefer an earlier communication to natural philosophers of the beginning of researches of this class; because at any rate these researches would advance more certainly, should they become the object of many observers, not merely for assembling scattered and unconnected phenomena, but for considering the light that they reflect upon each other, which may help to trace up their real causes. No spontaneous effects can manifest in a more characteristic manner these remote connexions between terrestrial phenomena by common causes, than those offered to our view by the atmosphere, in which, therefore, we must endeavour to extend our knowledge by meteorological observations: and as these phenomena have been for a long time one of the principal objects of my attention and study, I purpose to explain in the last part of this paper, the connexion that may exist between the indications of the aerial electroscope, when properly settled, and many atmospheric phenomena which are daily observed without being really understood.

Ashfield, near Honiton, 23rd August, 1810.

(To be continued.)

New Researches on the Properties of Electric Currents; Discontinued and directed alternately in Contrary Directions. By Professor M. A. De la Rive.*

(*Continued from page* 47).

SECTION II.

Passage of Magnetic Electric Currents through Metallic Conductors.

I HAVE found that the magnetic electric currents are better conveyed by a heterogeneous conductor, than by a homogeneous one. I here give the experiment to which I refer for support in this opinion:—"In traversing a conductor of the same total length, that is to say a metre, but formed of two wires attached end to end, the one of iron, the other of copper, the current gave 75° (by the thermometer of Breguet); by putting in succession to each other four ends, alternately *copper* and *iron*, but always maintaining the same total length, this current indicated 76°; it gave 77° when the conductor was composed of eight ends alternately *copper* and *iron*. In each case the total length of the conductor was the same, and the copper and iron wires were always of the same diameter—a millimeter."

I immediately add, "It is probably to the circumstance that the magnetic electric currents are composed of currents discontinued and alternately contrary, that their preference for heterogeneous conductors is due; whilst that voltaic currents and thermo-electric currents continued, are always travelling in the same direction, traverse with much greater facility homogeneous conductors."

M. Lenz has made these experiments, but as I have said, with a single instantaneous current, which made the needle a galvanometer; he has not observed any difference in the intensity of the current in passing through the ends of an iron wire, and the ends of a copper wire of the same length, placed one after the other; whether these ends were alternated, or whether they were not alternated. I should not have been surprised if the author had found a contrary difference to that which I had observed: in fact, it is well known, by my own experiments and by those of M. Peltier, that the change of the conductor (when even the conductors are both metallic) is, for a current constantly travelling in the same direction, a cause of diminution. These experiments of M. Lenz prove that there is no longer a diminution when the current is instantaneous: mine prove that there is a slight augmentation, when, in place of an instantaneous current, or of a continuous current, we make use of a series of instantaneous currents, alternately moving in contrary directions, which traverses the system of solid conductors alternately between them. The first of these results, proves probably, that an instantaneous current, on account of its enormous

* *Archives de l'Electricité*, No. 1.

force of projection, is not, like a continuous current, sensible to the small differences of conductibility which are due to the changes of solid conductors. What I have advanced is not a pure hypothesis; for how can we explain, if we do not attribute to the same cause, the fact that an instantaneous current produces physiological effects so powerful, in spite of the great resistance which ought to be presented to it by the introduction into the totally metallic circuit of an organized body; whilst that a pile whose circuit is a far less good conductor produces such feeble results comparatively speaking? A series of discontinuous instantaneous currents ought to present the same property as an isolated current; but if these currents move alternately in a contrary direction, not only are they less sensible to the change of conductors, but these changes appear to facilitate, in a small measure, their transmission. I do not know how to see any contradiction between these results; and as to that which I have obtained, it is perhaps premature to attempt to explain it; and I have also abstained from it in my memoir. Nevertheless, I shall take occasion to show that this phenomenon presents nothing contrary to received ideas, and to indicate the cause to which it appears to me to be due; at the same time, always recognizing the fact that this subject still merits to be well studied.

A primary current instantaneously determines in a conductor which it traverses, two currents by induction; the one *a*, moving in a contrary direction to its own which is simultaneous with it, and which consequently diminishes a little its own intensity; the other *b*, moving in the same direction, and which succeeds it immediately. This current *b* is simultaneous with the second instantaneous current which immediately follows the first; and which moves in a contrary direction, *par consequence*, also in a contrary direction to the current *b*. This one diminishes then the intensity of the second instantaneous current: this second current in its turn determines currents by induction, which ought to produce the same effect on itself, and on the third instantaneous current; and so of the rest. It results then from that which precedes, that a series of instantaneous currents travelling in alternative contrary directions through a conductor, determine currents by induction in this conductor, which diminish their original intensity. All that will enfeeble these currents by induction, will as much augment the intensity of the fundamental currents: now, this weakening, we may produce by disposing the metallic wire in such a fashion that the ends of iron and copper, of which it is composed, are alternated as much as possible; this disposition, in fact, enfeebles excessively the induction of the current on itself, which is, as we know, as much stronger as the homogeneous conductor in which it takes place is increased in length.

Such is then the explication which appears to me the most natural of the phenomenon which I have observed, and which as we see proceeds from the fact that the currents with which I have obtained

it are instantaneous, and which move alternately in contrary directions.

SECTION III.

On the Passage of Magneto-Electric Currents through Liquid Conductors.

M. Lenz studied in detail the paragraph in which I was occupied on the passages of magneto-electric currents through liquid conductors. He believes, as I do, that the influence which the length more or less great of the liquid conductor exercises on the intensity of the transmitted electricity, is greater for these currents than for magneto-electric currents: he remarks that it is a consequence quite natural of the law of Ohm. I do not deny it, for here you will see what I add after having reported my results.

"I do not endeavour to seek in the preceding tables the following law which the intensity of the current augments in proportion as the distance is diminished; this law, in fact, is connected, as will be shown, of the beautiful works of M. M. Ohm and Fechner, with the conductibility of the whole of the circuit which the currents traverse, and consequently, in this case, with that of the metallic wire in which it is developed, and of the helix which it traverses, &c."

M. Lenz is no longer in agreement with me when he attempts to appreciate the influence which is exercised on the intensity of magneto-electric currents by the interposition of a metallic diaphragm in the liquid which the currents traverse. I have remarked that, if this diaphragm has an extent equal to that of the section of the liquid in which it is placed, the magneto-electrical currents do not undergo any loss of intensity, because we do not charge the length of liquid traversed; whilst that, in the same circumstances, the voltaic and thermo-electric currents undergo a sensible diminution. M. Lenz concludes, either from experiments which he had made precedingly, or from new ones which he has made in the same circumstances as mine, that is to say, by interposing a plate of platinum in a diluted acid, that an instantaneous current will undergo a considerable diminution of intensity by the effect of the interposition of a metallic diaphragm in the liquid conductor. He finds even that the resistance which the column undergoes in its passage across the plate, is nearly ten times as great as that which it meets with in traversing the entire body of liquid.

I am ready to recognise the exactness of the result obtained by M. Lenz, but I do not conclude from it that mine are erroneous. In fact, our experiments are not comparable: since in the one a single instantaneous current is made use of, and in the other a series of instantaneous currents is employed, alternately travelling in contrary directions. It is true that the nature of the circuit, quite metallic, in which the magneto-electric currents are developed, ought to render them much more sensible than the hydro-electric cur-

rents to an augmentation of resistance in the remainder of the circuit which they have to traverse, such as that which results from the interposition of a diaphragm: this is what M. Lenz found. But when it is a series of currents moving alternately in contrary directions which is transmitted, a phenomenon is exhibited which must singularly modify the results. The metallic plate of which I have made use is of platinum; it receives successively on each of its faces the oxygen and hydrogen proceeding from the decomposition of the water brought about by the successive currents, and yet there is no disengagement of gas shown, as I have said in my memoir. This absence of gas is due to the oxidation and successive restoration which the platinum undergoes, as I shall show in the second part of the present memoir. Now then this chemical action makes the resistance disappear which a metallic diaphragm exercises on the currents which traverse it. I have shown, in fact, in the preceding researches,* that the chemical action which the surface of a diaphragm undergoes, diminishes the resistance which this diaphragm opposes to the transmission of electricity; and in the case of which we are now speaking, each thread of the current bringing with it its oxygen, which is afterwards borne away by the hydrogen of the following current, it is easily transmitted by the successive oxidation and the de-oxidation which thus operate. Also, as soon as a little gas shows itself, the chemical action is no longer complete on the platinum plate, and the resistance which it opposes to the transmission of currents, then becomes visible. A very great number of facts confirm the exactness of this explanation. These facts are already made known in my published memoir. I shall have occasion to cite them anew in the second part of the present one.

SECTION IV.

Influence of the Size and Form of Metallic Conductors which ought to conduct the Currents in a Liquid.

I have devoted a paragraph to the study of the influence of the form and extent of a metallic conductor which serves to transmit the currents in the liquid. The most important result which I have obtained is, that when the extent of a metallic conductor goes beyond a certain limit, we do not augment the intensity of the currents transmitted in augmenting this extent. When the limit in question is attained, there will be no development of gas on the surface of this conductor; consequently all chemical action appears to cease, though the transmission of the electricity is effected with greater facility than it was when the surface of the conductor being less extended, bubbles of gas were seen to appear there. I have attributed the effect which I have just described, to the fact, that the magneto-electrical currents being totally transmitted when we attain

* *Ann. de Ch. et de Phys.*, tom. xxviii, p. 213, et tom. xxxvii, p. 251.

a certain extent for the surface of contact between the solid and liquid conductor, we no longer gain anything by augmenting this extent. As to the cessation of the disengagement of gas, I have thought that it might possibly be explained by the enfeebling which the threads of the current undergo in being disseminated, which enfeebling renders them incapable of producing chemical effects; I have added that these effects always manifest themselves in those points where the currents are confined in their passage; as soon as they no longer experience this constraint, which happens when we attain the limit in question, the phenomena of decomposition can no longer take place. I have afterwards cited some facts relative to the influence of the form of the conductor, and I have afterwards terminated the whole of the paragraph by the following reflection:

"It is possible, as we shall see further on, that the slight chemical action, which may in some cases take place on that part of the surface of platinum in contact with the liquid in which the currents of electricity are transmitted, was not without some influence in the production of those phenomena which have been the object of this paragraph."

M. Lenz candidly admits that the extent of the metallic surface which transmits any current whatever in the liquid, ought to enfeeble the elementary threads, and perhaps render them incapable of producing chemical decompositions; but he is not convinced that the absence of chemical actions is quite real, he supposes that it is perhaps only apparent, and that the bubbles of gas disengaged are so small that they rest dissolved in the liquid or are invisible. He presumes also that, in the experiments which I have made, the disappearance of the gases may proceed from the combination of the oxygen and hydrogen on the surface of the platinum, where they arrive so rapidly one after the other. I am bound to acknowledge that I am led to believe that M. Lenz is right; already, as the paragraph which I have above cited will prove, I have expressed doubts on the real absence of chemical action in the case where no disengagement takes place. And even after having shown that when the surface of the conductor becomes very great, the chemical action of a voltaic current diminishes also, but without becoming completely null, I have added the following phrase:—"This point merits a new examination, and I reckon upon returning to it incessantly, in especially studying in view the voltaic currents and the influence which may be exercised on the phenomenon, not only by the extent, but the different nature of liquid and metallic conductors in contact, through which the current is transmitted, and where the chemical decomposition is effected."

Some new observations which I have since made, and which are contained in the remaining part of this memoir, have shown me, in fact, that there is an undoubted chemical action when even there is no sensible disengagement of gas on the surface of conductors which transmit the currents through the liquid. Thus the idea

which I had advanced, that these currents produce chemical action only when they are confined in their passage, appeared to me to call for a new submission to examination; it is possible that the chemical action is only weakened without ever becoming completely null.

As to a limit in the extent of the surface of contact between the metallic and liquid conductors, the most favourable to the transmission of currents, M. Lenz contests it. He cites experiments always made with the instantaneous electric current, from whence it results that he has always augmented the intensity of the current transmitted in proportion as he gave more extent to the surface of the metal in contact with the liquid. However, he acknowledges himself that the augmentation of the intensity which corresponds to a certain augmentation in the surface immersed, is no longer so sensible when this surface is already very great; but he affirms that it always does take place. I will remark on this point that M. Lenz has not sufficiently augmented the surface of the immersed metal to attain the limit which exists, I believe, with every species of currents; but I ought to observe that with currents propelled alternately in contrary directions, the limit ought to be much more quickly attained, because of the chemical action of which I have spoken previously, which facilitates the passage of the electricity of the liquid in the metal, and reciprocally of the metal in the liquid. Whatever be the cause of the existence of this limit, it is evident that there is one, at least of the currents of which I have made use; since if we go beyond it, even very far, we do not augment either the chemical effects or the calorific effects of these currents; whilst, previously to attaining it an augmentation in the extent of the immersed surface sensibly increases the energy of these effects. M. Lenz here made me an objection which I have not comprehended: he says that if I attain this limit, all the current ought to pass, and that I ought consequently to obtain then the same calorific effect as I obtain when the circuit is entirely metallic. This would be true, if all the resistance which the currents meet with in their circuit had existence only at the passage of the metallic plate in the liquid; but the liquid itself exercises one which remains constant, and we attain the limit when we have annulled the first of these two causes of resistance, the second always continuing to subsist. This is what I express in saying that all the magneto-electrical current is transmitted, that is to say, all the current that can be developed in a circuit partly metallic and partly liquid.

SECTION V.

Particular Phenomena which take place at the Surface of the Metals which have served to put the Liquids in the Magneto-Electric Current.

M. Lenz has made no remark on the phenomena which form the object of this paragraph, and which constitute the most important

part of my memoir. He only observes that it is probable that they may be produced by hydro-electric currents placed in the same conditions as the magneto-electric currents; I am in this respect altogether of his opinion, and I call to my aid even more than he does,—experiment to the support of my opinion.

The phenomena now in question are referable to the formation of a metallic layer or bed, finely divided, on the surface of the metals which transmit into a liquid the magneto-electrical currents. This effect appears to be due to a series of oxydations and reductions which the surface of the metals undergo, even that of metals the least oxidable in appearance, such as gold or platinum. I do not insist for the moment on this category of facts, to which I return in the remaining part of my memoir.

SECTION VI.

On the Effects occasioned by the simultaneous employment of Liquid and Metallic Conductors, for the Conduction of Magneto-Electric Currents.

I have explained in this paragraph some facts which seem to show a species of interference exercised by the magneto-electric currents on each other. Before arriving at the helix of the metallic thermometer, the currents traversed two conductors situated parallel to each other, between which they were divided: the one of these conductors was a very thin plate of dilute sulphuric acid, placed between two very broad plates of platinum; the other a metallic wire whose length could be varied at pleasure. Now I observed that the addition of this second conductor not only did not augment the calorific intensity of the currents on the helix, but that they might even diminish it by giving it a certain length. I have indicated the lengths which produced, with wires of different natures, and the same diameter, the maximum and minimum of effects.

M. Lenz has made a great number of experiments on this subject: but he has always employed a single instantaneous current; he has made it pass through two parallel conductors, sometimes both solid, sometimes one solid, the other liquid. He has constantly observed that, conformably to the law of Ohm, the current was divided between the two conductors, proportionally to their conductibility, and that the conductibility of the double circuit was the sum of the partial conductibilities of the two parts of which it was composed. The solid conductor was, in his experiments, a metallic wire of greater or less length, and of a variable nature; the liquid conductor was of sulphate of copper, placed in the circuit by means of two plates of copper.

It is not astonishing that M. Lens, having made his experiments under conditions altogether different to those under which I made mine, did not arrive at the same results. I observed first that in

my experiments the conductor, formed of a thin layer of diluted sulphuric acid, placed between two plates of platinum, was as good, and even better, than the conductor totally metallic, formed of a fine silver or platinum wire. This was not the case, I believe, in M. Lenz's experiments, where the solid conductor was better than the liquid one. Besides, I made use of currents transmitted alternately in contrary directions, and M. Lenz employed only an instantaneous current. Now it is very possible that the phenomenon which I observed belonged to this second circumstance. It is a point which merits being cleared up; and generally speaking, the entire subject has to be studied. I have engaged to make this study by publishing the first results which I have obtained: I will fulfil my engagement. I will willingly agree that I have perhaps been too hasty in speaking of interference, before having compared a great number of observations; though I persist in maintaining the exactness of my experiments, when we operate in the same conditions as I myself have operated, and to find that they seem not sufficient to be explained except by a species of interference.

I terminate here this first part of my memoir. It will perhaps be found that I have treated it with too little development; but the work of M. Lenz is a work very long and full of details, and it is impossible for me to leave him without saying so; since it has had for its exclusive object the criticising of my preceding researches. It is to be regretted that M. Lenz did not make, before undertaking this *critique*, the reflection by which he has terminated it: the result would have been a much shorter controversy between him and me.

This is what he says after, as I have above referred to, combatting the conclusions which I come to in the Section VI:

"As to knowing if a rapid succession of magneto-electric currents, opposed among themselves, modify the phenomena which are due to the influence of conductors, that is what other researches may decide. In any case, the cause is not in the nature of the currents, but in the mode of their succession; the phenomena will be the same if a hydro-electric current were conducted similarly in directions alternating contrary through a conductor, by means of a commutation."

I have never said anything else: thus, to combat my results it was necessary to place himself on the same ground that I had taken, and consequently to make use of the same currents as I have made use of.

(To be continued).

MEETING OF THE BRITISH ASSOCIATION.

(Continued from page 80).

Mr. Hutton then read the "Report of the Committee for Registering the Shocks of Earthquakes in Great Britain." The report described the instruments which had been used, and in whose charge they had been placed. During the period of eleven months, out of

sixty shocks felt, only three of the instruments were affected. On the 30th July, 1841, a shock was felt at Comrie, when the pendulum of one of the instruments used, vibrated east and west. The ground vibrated considerably, and the chimnies of some houses a few miles from Comrie were thrown down. Mr. M'Farlane, who had the care of the instruments, describes the chimnies of some of the houses, and also the walls, as being rent, and that, generally, the walls so rent ran in a north and south direction, whilst those running east and west escaped. It was supposed that the particular spot from which the shocks emanated was at the distance of a few miles from Comrie, and the committee urged upon the association the desirableness of making further observations, in order to ascertain the cause of such shocks, and their frequent occurrence in Perthshire. The earthquakes were supposed to be owing to the agency of electricity, and were believed to have some connexion with the state of the weather. The committee urged upon the association to provide additional instruments for making meteorological observations, and expressed a hope that the efforts of the committee would be successful. After the paper had been read, thanks were voted to Dr. Buckland and Mr. David Milne, by whom it was communicated. Dr. Buckland then made some observations on the importance of having instruments to register the operations which are now going on in the bowels of the earth, and also to ascertain the causes of the other phenomena attending earthquakes. Within two years one hundred and seventy shocks of earthquake had been felt in the lower line, of country of Perthshire, parallel to the Grampians. He referred to the late earthquake in St. Domingo, and to the probability of the same phenomena being more generally exhibited in this country.—Professor Sedgwick, after expressing his opinion that the state of the atmosphere was not so likely to have an effect in producing earthquakes, as the phenomena attending the latter were likely to affect the state of the atmosphere, expressed a hope that the association would, notwithstanding, provide all the requisite instruments for making meteorological observations.—Dr. Buckland said that what was called the earthquake season in Perthshire, was the rainy season in the winter months, and on that account the atmospheric phenomena had been supposed to have some effect in producing these small earthquakes; as for instance, through the infiltration of water.—Mr. Nicholson, of Kendal, gave some account of an earthquake which occurred at Morecombe Bay, on Sunday week. On that day the thermometer was higher than it had ever been since 1826. He drew no inferences from that fact, although it did seem to form a connection between the state of the barometer and the earthquake itself. There had been great drought for some weeks previous to the earthquake, and rain came on the following day.—A member of the association, whose name we could not learn, observed that earthquakes at Comrie were of recent origin, none having been known to take place prior to 1780.

A communication, by Dr. James Starke, "On the Structure and Mode of Formation of Glaciers," was next read. The paper gave an account of the discoveries that have been made by different scientific individuals respecting glaciers on the Alps, and other places. The author is opposed to the theory of M. Agassiz, respecting the origin of glaciers, from whose description of the glaciers in Switzerland, and also from the writings of Dr. Scoresby and others, who have visited the Polar regions, he arrives at the conclusion that glaciers are generally stratified in a vertical and longitudinal direction, and have been formed by the freezing of daily falls of snow. The granular and other variations from the vertical and longitudinal direction of the strata, happened in various ways. The granular appearance of the glaciers he supposed to be owing to large masses of ice falling over the barrier into the valley beneath, by which it was broken into fragments, and the water going over it a portion became solidified. Glaciers, he was of opinion, washed from an upper surface to a far greater extent than had hitherto been supposed, even from above what had been called "the line of perpetual snow." It had always appeared to him that there was scarcely a phenomenon in nature which might not be illustrated by something in our own country, and he had often thought that the formation of glacier stratification might be illustrated by reference to ice upon a small stream.—The Chairman said he had the pleasure to return thanks to Dr. Starke for the communication that had been read. There was a number of gentlemen who might wish to take part in the discussion upon it, and he submitted to Dr. Buckland, who had given a great deal of attention to the subject, whether, as it was now within a few minutes of the legal hour (three o'clock), it would not be better to adjourn the meeting until to-morrow morning?—Dr. Buckland thought the subject was not geological, but belonged to the physical section, as the question related to the construction of ice. The author, it appeared, had never been on the Alps, and was not, in his opinion, entitled to take those high views which he had done. There were gentlemen present who had been in the icy regions, and he thought, as they might not be present to-morrow, if they wished to make any observations, now was the time.—The Chairman agreed with Dr. Buckland that the subject was much better calculated for the physical section, and therefore would hand over the paper to Sir David Brewster, who might make what use of it he thought proper.—After a few words from Dr. Richardson and Lieut.-Colonel Sabine, the meeting adjourned.

SECTION D.—ZOOLOGY AND BOTANY.

President, the Dean of Manchester.—This was an interesting meeting. The papers read were, the "Report of the Committee for the Preservation of Animal and Vegetable Substances," whose experiments had continued for four years. They found as good preservatives subcarbonate of potash and naptha, employed in the

proportion of one part to seven of water. Moderately good, but the specimens were rather. too soft, were sulphate of magnesia and arseniate of potash. Moderately good, when examined in 1840, but the specimens now decomposed, were alum, muriates of ammonia and magnesia, nitre, sulphate of zinc, bi-carbonate of potash, and arsenite acid. The various sorts useless for the purposes of preservation :—a few drops of kreosote in water is a good preservative, but stains the specimens brown, whilst corrosive sublimate preserves perfectly, but hardens the substances too much.—Mr. Blackwall read a long paper "On the Palpi of Spiders," the result of many observations on living specimens.—Dr. Richardson exhibited a specimen of *machærium subducens*, from Port Essington, New Holland; a remarkable fish, new to naturalists.—Dr. John Richardson, of Haslar, read a long paper "On the Present State of the Ichthyology of New Zealand." In many respects the zoology of this country is singular; quadrupeds being very rare, Captain Cook having only discovered the dog and rat, whilst Mr. Polak states that it nourishes no serpents nor snakes of any description. Upwards of one hundred varieties were described, many of which were new to ichthyologists.—Mr. Moore exhibited a collection of parasites, found in the carp and salmon, which gave rise to an interesting discussion.

SECTION F.—STATISTICS.

The section was thinly attended, only about twenty gentlemen being present. This was probably owing to the fact that no business had been announced in the programme as likely to come before the section on that day.

Spade Husbandry.—The first paper read was one by Mrs. Davies Gilbert, widow of the late D. Gilbert, Esq., Secretary to the Royal Society. The paper was read by Mr. G. Webbe Hall, Bristol. It contained a statement of the advantages of small allotments of land, when cultivated by labourers, and also an account of the introduction of the system into a charity school at Winnington, near Eastbourne, in Sussex, by which the labour of boys for three hours a day, was made to pay for their instruction. The method of stall-feeding cattle was also recommended as superior to the grazing system, and extracts were introduced from the *Farmer's Magazine*, and *Penny Cyclopædia*, illustrative of the advantages produced by this system in different places in the south of England, where a number of families had been rescued from pauperism. Applause followed the reading of the paper, and thanks were voted to the authoress.—Mr. Porter, of Ireland, said he had had much to do with this species of culture, and he could testify to the superior advantages of liquid manure and stall-feeding. He thought the suggestions thrown out in the paper most valuable.—Mr. Woollcombe expressed similar sentiments, as also did Colonel Sykes, who avowed his astonishment at the results recorded in the paper, and trusted that they would become generally known throughout the kingdom.—Mr.

Felkin, of Nottingham, spoke of the advantages of the division of land on the continent, particularly in Saxony, where the comforts of the poor manufacturing operatives were thereby considerably enhanced.—Mr. Webbe Hall, who had been called on by the chairman, to give his opinion as to the practicability of the methods suggested, said it was clear that the system was one founded on the acknowledged principles of human nature; an appeal to which, rightly made, must ever be successful. He could testify that the results stated were quite within the bounds of possibility, probability, nay, even certainty.—The Chairman remarked that the paper showed the value of co-operation between landlords and tenants, and the importance of combining industrial employment with the instruction of youth. The system would be a great boon to Ireland, though he thought, with Mr. Webbe Hall, that it would be visionary to cut up the whole of England into small allottments; that would be a retrocession, rather than an advance.—Colonel Sykes remarked that the system was evidently applicable on a large scale, as the authoress of the paper spoke of employing 300 labourers in that way on her farms.—Mr. Porter gave some further account of the operation of the system on his estates in Ireland, the introduction of which had been suggested to him by a former paper of Mrs. Davies Gilbert.

Supposed Influence of the Factory System in Developing Consumption.—Mr. D. Noble, surgeon, of this town, next read a paper on this subject. He observed that the influence of the factory system in the deterioration of health and the production of disease, had been a subject of great diversity of opinion. It had been denounced in the strongest terms by physicians of the highest reputation, and had been reproached as destructive of every sound condition of the body, and as the fruitful source of consumption and scrofula. Before a committee of the House of Commons, obtained some years ago, by the late Mr. Saddler, these views had been very unequivocally expressed by several metropolitan members of the profession, and one witness had made bold to assert that scrofulous diseases were immeasureably more abundant in Manchester than in the metropolis and other places, and that the proportion in which such cases were developed could not be less than one in every ten individuals. Others had gone into the opposite extreme, and maintained that the factory labour was a true protective from consumption and scrofulous affections, and that, generally speaking, such deviations from sound health were less frequent here than elsewhere. Amidst such a diversity of opinion on a matter of fact, it might be difficult to determine the real state of things; but it was quite certain that many intelligent persons, both medical men and others, did regard working in factories as giving rise to pulmonary consumption, or as inducing it where a strong predisposition existed. Most of these founded their opinions on some theory, but the deductions from theory might be erroneous; and it was an interest-

ing subject of inquiry whether such was the case. He proposed, first, to examine to what extent the general results obtained by the registrar-general of this country, confirmed the idea that pulmonary consumption was more prevalent in Manchester than in other large and populous districts. He should compare this with other towns where few or no manufactures existed; and should then subjoin some of the results of an examination which he had made, in conjunction with Mr. Ner Gardiner, of the registration books of this town, for the last three years: data which would serve to show the proportion which the whole number of cases of consumption occurring in this place bore to those of persons employed in factories. These statements might tend to elucidate the inquiry as to the special agency of factory employment in cases of consumption. He would first submit a few remarks as to the nature of the evidence furnished by our national system of registry. In very minute medical inquiries on special pathology, little or no aid could be drawn from this source; but when the object was to determine the general character of the diseases prevalent in various parts of the kingdom, and to estimate the influence of outward causes in the production of fatal disease, the facts to be gathered from the registration books became of great importance. The causes of death being generally recorded on no better authority than that of the parties giving information to the registrar, parties who were unprovided with any medical certificate, and who gave their answers to the questions in popular phraseology, an intimate knowledge of which phraseology was necessary in examining the registries; yet in spite of these drawbacks, very satisfactory and practical useful results might be obtained. The numerical statement he was about to submit had been taken from the third and last published report of the registrar-general, and the figures applied exclusively to the year 1839; but, on a comparison of this with the foregoing reports, he found a remarkable accordance in all the main results: an accordance which furnished the strongest proof of the importance of these documents in a statistical point of view. According to the census of 1831 (that of 1841 not having been obtained at the period of the publication of these registration reports), there were 49,392 families resident in Manchester and Salford, and the entire number of deaths in 1839 was 9,223, of which 1,451 were recorded as having been from consumption. This, rejecting fractions, was at the rate of one death annually from consumption in 34 families, and, in proportion to the deaths from all causes, those from consumption were 3 to every 19. This truly furnished very decisive evidence as to the prevalence of consumption in this district, and, taken by itself might seem to afford evidence that factory employment, so prevalent here, was in a great measure the instrument in producing this state of things. For, taking Essex, a purely agricultural country, he found that in 1831, it contained 62,403 families, exceeding by 13,000 the number in

Manchester and Salford, while the deaths from consumption were less by upwards of 250, being only 1,201 in 1839. Were the inquiry to rest here, an inference would almost inevitably be drawn, that the appalling statements as to the undue prevalence of scrofula and consumption were strikingly corroborated, and factory labour would be referred to as the cause of these things. It ought, however, never to be overlooked, that in large towns, a variety of causes, from which rural districts were exempt, were continually in operation, such as a clôse atmosphere, confined dwellings, cellar residences, irregularity of employment, and extreme variations in the rate of wages; all which circumstances considerably increased the general mortality, rather than that from consumption in particular. For the total number of deaths in Essex was 6,009 less than in Manchester and Salford, and the cases of consumption were 1 for every 52 families; from which statement it appeared, that in relation to the number of deaths from all causes, those from consumption were actually fewer in this manufacturing district, being in Essex as 4 in 21, while here they were as 3 in 19. Comparing it with another rural district, Cambridgeshire, Huntingdonshire, and the southern part of Lincolnshire, comprising 167,000 families, he found there that the deaths from all causes were 7,306 in one year, and those from consumption alone 1,308, or 1 in every 5; showing, as in the case of Essex, in comparison with this district, a greatly reduced rate of mortality in general, but not quite a corresponding low rate of deaths from consumption. But in estimating the condition of Manchester and Salford as to the extent to which consumption prevailed, the object being to ascertain whether factory labour was prejudicial, it was better to institute a comparison between these towns, and others similarly situated, except as to factory labour. For this purpose he had taken Liverpool and West Derby, a district much like our own in extent and the general character of its inhabitants; and subject also to commercial fluctuations, but free from the evils, real or supposed, of factory labour. In 1831 there were 43,026 families in Liverpool and West Derby, being 6,000 below those of Manchester and Salford; in 1839 there were 9,181 deaths, approaching those in Manchester and Salford within 42, while the deaths from consumption were 1,762, or 300 in excess of those with us, notwithstanding our greater population. At this rate 2 deaths from consumption annually occurred in Liverpool in every 49 families, and the same number here in every 68. The deaths from consumption in Liverpool, as compared with those from all causes, were 4 in 21, while in Manchester they were only 3 in 19. Birmingham, another large town exempt from factory operations, might be drawn into comparison. By the census of 1831, it contained 23,934 families; in 1839, the registered deaths were 3,638, and those from consumption 668. These numbers furnished somewhat more favourable evidence of the value of life, and the exemption from consumption,

than those of Manchester, but only to a slight extent; there being 1 death from consumption in every 36 families, and 2 in every 11 of the whole registered deaths. The metropolis furnished a still smaller proportion of deaths from consumption, being 2 for every 105 families, and 3 in every 19 of the deaths from all causes: the same rate as in Manchester. Therefore, as compared with other localities, Manchester and Salford seemed, on the whole, rather more exempt from this disease than some other places; decidedly more so than Liverpool, but in comparison with the agricultural districts and the metropolis, more subject to it. It was a remarkable fact, however, that the metropolis excepted, Manchester had fewer deaths from consumption, compared with the whole number of deaths, than any other place. Thus here, as in other instances, the facts led to a conclusion different from that arising from vague and popular impression. It might, however, be supposed, that in the deaths occurring from consumption, the factory cases furnished an undue preponderance. To ascertain this he had consulted the death registries of 1838—39—40, and had taken therefrom the age and attested occupation of all parties registered as dying of consumption or decline between the ages of 15 and 40, in the township of Manchester, which comprised a fair share of the factory population, and furnished a fair type of the whole. He had confined himself to the ages between 15 and 40, as most likely to exclude errors from improper entries of the cause of death. The township of Manchester, with a population of 160,000, and an annual average of 6,000 deaths, afforded 1,141 registered deaths of consumption in those three years. Of these, 174 were individuals working in factories, 590 persons of various occupations, and 377 without any attested employment, chiefly wives and children not attached to any particular pursuit. Of the 174 inmates of factories, there were 45 spinners, 49 winders, 28 piecers, 15 reelers, carders and frame tenters 11 each, 10 were stated to work in factories without mentioning the precise occupation, and of the remainder, doublers, stretchers, beaters, &c., none exceeded 5 separately. He had excluded from this calculation weavers, and had included other designations which were somewhat ambiguous as to whether the parties were employed in factories or not. It was not necessary to know the ratio of the factory population between 15 and 40 to the entire inhabitants; the rate of mortality from all causes in the two classes, and the rate of mortality from consumption authenticated by medical certificate; but in the absence of these materials some approximation to the real state of the case might be formed. Considerably below one sixth of the 1,141 dying from consumption, namely, 174, were, during their lives, engaged in factory labour; and he did not think it too much to say that of the whole of the inhabitants between 15 and 40, not much less than one-sixth were employed in factories. If so, no confirmation was given to the supposition that consumption was unduly prevalent amongst the factory population. It

might be assumed, however, that factory labour wasted the energies, and induced a variety of diseases. If so, such mortality would be more manifested in the cases of decline. He had classified the ages of the 1,141 in periods of five years, and found that there had died between 15 and 20, 195; between 20 and 25, 243; between 25 and 30, 260; between 30 and 35, 223; and between 35 and 40, 220. On comparing these numbers with those exhibited by the general statistics of consumption, given in Sir James Clarke's tables, he found a remarkable concordance. The general conclusion resulting from the above facts would certainly appear to be that manufactures exerted no unusual agency in the production or premature developement of pulmonary consumption, in opposition to the general opinion, both by intelligent persons and others; for, it appeared from the mortuary statistics of this metropolis of manufactures, that it was less liable to consumption than Liverpool, and the relative numbers had scarcely differed from those of Birmingham, whilst with the metropolis we contrasted less favourably; and though we were in excess above the agricultural districts, this appeared to be the effect of other circumstances rather than of the existence of manufactures.—The thanks of the section having been voted to Mr. Noble, Dr. Arrowsmith said he had no doubt of the correctness of the data. He believed that where regularity of life prevailed, it was more conducive to health than otherwise. At the same time it must be borne in mind that the present method of keeping registers lead to great errors as to the causes of disease; and he suggested a method for its improvement by having medical certificates as to the cause of death.—Col. Sykes observed, that the general rate of mortality in Manchester, which appeared by these documents to be about 1 in 25, showed that factory employment was less healthy than some others, rural employment, for instance. Though the proportion of deaths from consumption, and those from all causes, were the same in London and Manchester, yet the average rate of mortality in London was only 1 in 37.—Mr. Noble said the rate of infant mortality was very great in Manchester, nearly half the deaths being of this kind.—The Chairman mentioned some circumstances as tending to explain the apparent low rate of mortality in London.—A gentleman, whose name we did not learn, pointed out a remarkable coincidence, that the rate of mortality in Manchester exceeded that of London, by about the same rate as that of Glasgow exceeded Edinburgh.

SECTION G.—MECHANICAL SECTION.

Professor Willis took the chair in this section, supported by Sir John Robinson, Sir Mark Brunel, Mr. Vignolles, and several gentlemen resident in the neighbourhood. After briefly opening the proceedings, and detailing the business for the day, the Chairman announced that the following day would be devoted to papers and discussions on the prevention of smoke, and the fusing of metals;

Saturday to ships and steam engines; Monday to subjects connected with railways; and the subsequent days to such miscellaneous subjects as might arise, or to such of the previous topics as might not have been exhausted on the days specially appropriated for their consideration. The Chairman then called on Mr. Vignolles to read a report of the committee on railway sections.

Mr. Vignolles proceeded to read the report, which stated, in substance, that, at the Glasgow meeting, on the joint application of the mechanical and geological sections, a grant of £200 had been made for the purpose of obtaining profiles of the different railways constructed throughout the country, before the slopes of the excavations became soiled over, or covered with vegetation, with the view of putting on record the strata and the geological appearances developed by the extensive operations of modern engineering over so large a portion of the island. Of this grant, which was renewed at the Devonport meeting last year, the whole amount had been expended; and the results were then before the sections, originating the subject in the shape of numerous working plans and sections of several of the railways in this neighbourhood. The committee had received the most effectual and willing assistance from the officers of the different companies, and especially from the engineers; and the committee could not refrain from particularly mentioning Mr. Swanwick, the engineer of the North Midland Railway, who, as the works of that line proceeded, had carefully marked on his working plans all the geological details of the cuttings, passing through a most interesting region,—presenting an example of the combination of engineering and geological information well worthy of imitation by other engineers. The committee had found, after some experience in working the grant entrusted to them, that, with the facilities which were afforded to them by the different railway companies, they might, without increasing the expense, be able to form an interesting and valuable collection, not only of sections of the excavations, but of the plans and sections of the different railways; which, when concentrated into a public depository, accessible to all literary and scientific men, would be found exceedingly interesting. Indeed, it might be hoped, that the subject would eventually be taken up by government, and the information thus collected made to form part of the geological survey of the country, in connection with the trigonometrical survey now carrying on by Col. Colby. The committee, therefore, hoped that the two sections would apply to the general committee for a further grant, to enable them to complete the organization they had begun. The documents which the committee had to submit, as the fruits of their labours, were:—1. Plans and sections of the North Midland Railway, from Derby to Leeds, about 72 miles, with the whole of the geological details laid down on the section of the cuttings.—2. Plans and sections of the whole of the Midland Counties Railway, about 68 miles.—3. Plans and sections of the Manchester and Leeds Railway, from Manchester to Normanton (not yet quite fiinished),

with considerable portions of the geological details filled up.—4. Enlarged sections of the Glasgow, Paisley, and Greenock Railway, with the geological details.—5. Enlarged sections of the Manchester and Bolton Railway, with full details of the strata where the extraordinary fossil trees were found; and of the trees themselves (models of which are in the exhibition-room at the Royal Institution, Mosley Street).—6. Enlarged sections of the Hull and Selby Railway, with the geological details. The sections already received comprised between two and three hundred miles of railway; and some others were stated to be preparing, but they had not come to hand. All these records, according to the directions of the Association, were to be placed in the Museum of Economic Geology in London, where they may at all times be usefully referred to. The report concluded with some striking and just remarks on the value and importance of the information thus collected, to the philosopher, the geologist, or the engineer.—Thanks having been voted to Mr. Vignolles, for the report, Mr. Bateman said that Mr Binney, of this town, was now engaged in ascertaining the geological details of the Liverpool and Manchester Railway, one of the cuttings of which exhibited the coal measures breaking through the new red sandstone, presenting some very striking appearances, which, though the railway has been opened about twelve years, had been unaccountably neglected. If the strata had not been of a very sterile character, the appearances now presented would in all probability have been obliterated by vegetation.—Mr. Vignolles said he was glad to find, that other parties were engaged in the same pursuit as the committee; and he hoped that Mr. Binney would lay down his sections on the same scale as those adopted by the committee;—a scale of 40 feet to to an inch both vertically and horizontally; for which purpose the committee would be glad to furnish him with ruled paper that would greatly facilitate his labour. The Chairman said it certainly was highly desirable that all the sections should be on the same scale.

Self-acting Wears on Rivers.—Mr. J. F. Bateman, civil engineer, read an interesting paper on a new contrivance which had never yet been put in operation, and which he illustrated by a model, for avoiding the accidents and objections to which the wears at present in common use on rivers subject to floods or strong currents were liable. Mr. Bateman explained at some length the objections to which the present wears were open, the most formidable of which we understood to be the silting up of them, owing to the want of means for cleansing away the dirt brought down by the stream, and the liability of them to break down by the rising of the water. The plan he proposed is one by which two gates, hanging vertically across the river, the one above the other, the upper one overlapping the under one slightly, and keeping it in its position till the water attains a certain height, are made to act of themselves; the upper one as the water rises yielding to the pressure so as to open the lower one. By this means free vent is given for the stream to pass

under as well as over the wear, carrying with it, of course, the silt, which would otherwise accumulate in the bed of the river. The paper gave rise to a short discussion, in the course of which Sir M. I. Brunel, F.R.S., and Mr. Fairbairn, civil engineer, expressed their approbation of the principle, and the former thought it highly worthy of a trial.

Ventilation of Dwelling-Houses and Public Buildings.—Mr. Andrew Liddell, of Glasgow, read a paper on a mode of ventilation adopted under the direction of Mr. Fleming, a surgeon, of Glasgow, in different public and private buildings, in and near that city; one of them a large building, four stories high and occupied by a considerable number of the poorest families. This mode of ventilation, which consisted in drawing of the foul air from each room by a pipe leading to the chimney of a steam engine, had been attended with the most beneficial results as regarded the health of the inmates, and paticurlarly by a great diminution in the mumber of fever cases. A similar plan has also been applied, with the best results, to the cabins of the Princess Royal steamer, which plies between Glasgow and Liverpool.—Sir John Robinson observed, that it was highly satisfactory to find sound principles in regard to ventilation, making their way amongst the people of this country; but it was at the same time to be regretted, that ineffective plans should be resorted to, when the very best plans had been many years before the public. The mode of ventilating the Derby Infirmary, devised by Mr. Strutt, and published by Mr. Silvester more than twenty years ago, had exhausted the subject of ventillation; and since that time nothing new had been invented, nor had any real improvement upon Mr. Strutt's plans been devised by anybody.

On Fractures of the Axles of Locomotive Engines.—Mr. Vignolles read a paper, the object of which, he stated was to fix the attention of the scientific engineers and manufacturers of Manchester on certain circumstances connected with the late frightful accident on the Paris and Versailles Railway, originally caused by the breaking of one of the axles of an engine. After some remarks on the identity of the circumstances under which the accidents had occurred on the Paris and Versailles, and the Brighton lines, glancing at the much disputed question of the relative merits of four and six-wheeled engines, and pointing out the precautions which should be adopted in forging axles for railway engines and carriages, the paper stated that some of the French engineers had suggested that the source of the often unexplained breaking of axles on railways must be looked for in another quarter than the mere quality of the iron of which they were composed, or the care which had been bestowed upon its fabrication. M. François and Colonel Aubert, who had both read papers at the Royal Academy of Paris, on the subject of the late accident, attributed the fracture of the axle to the iron having become crystallized by the heat or magnetism, arising from the severe strains and blows to which it was exposed. In support of

this opinion, they stated that the axle was formed of the very best iron, and was of sufficient dimensions; but that the fracture exhibited a decidedly crystalline appearance. M. François stated in his paper, that he had made a long continued series of experiments, and had observed that a magnetic action on iron, in a state of fusion, would produce similar effects, and change the small and closely adhering particles into coarse and large crystals, depriving the iron of its compact character. He therefore inferred, that the action of heat or of percussion on the axles of railway engines, when moving at high velocities, might produce a magnetic action which would be attended with the same results. Both M. François and Colonel Aubert, seemed to be of opinion, that the only effective precaution was to change the axles of locomotive engines so frequently as not to give them time to undergo this crystalline change. In seeming corroboration of the opinions of these distinguished French engineers, Mr. Fairbairn had stated to him (Mr. Vignolles) that cold swaging of hammered iron would cause it to assume a crystalline form; and, if so, the shocks which the axles receive during rapid transit, might easily operate in producing this remarkable change of texture. Although this reference to the supposed change in texture of iron constituted, in our judgment, the most interesting portion of the paper, it contained, as we have already intimated, a number of other points of considerable importance. This paper gave rise to a long, animated, and most interesting discussion, but of a character so diversified and desultory as to forbid any attempt at a detailed or consecutive report. We shall, therefore, omit such portions of it as related to the comparative merits of cranked and straight driving axles, and of four and six-wheeled engines, and confine ourselves to an abridgment of what was said on the important point suggested by the papers of the French engineers. On this point Sir Mark Brunel said, that he had been involved in a railway accident at Rugby, in which two or three axles were broken, and he had observed that the fractures exhibited a crystalline appearance, more like that of cast than of wrought iron.—Mr. Hodgkinson said that some of his experiments on cast iron tended to show that a force which would produce a very slight deflection in a long bar, would to some extent impair its elasticity, and prevent it from resuming its former shape, which seemed to show that there was some change in its internal structure. His experiments were certainly made upon cast iron; but he had reason to believe that the same results would be experienced with respect to wrought iron.—Mr. Fairbairn observed, that it was a fact well known, that if iron bars were swaged below a blood-red heat, the iron would become crystalline, however fibrous and ductile it might previously have been.—Mr. Hawkins said that at a recent meeting of the Society of Civil Engineers, a very interesting paper had been read on this subject, and numerous speecimns of fractured iron had been exhibited, which, though undoubtedly fibrous originally, had become decidedly

crystalline by use.—Sir M. Brunel observed, that any one who considers the ordinary motion of a railway engine or carriage, at a high rate of velocity, would see that the axle must receive an immense number of blows like blows upon an anvil, and which would probably have the same effect upon the texture of the iron.—Mr. Jeremiah Garnet said it was well known that violent hammering or friction rendered iron magnetic, causing it to attract iron filings very freely; and it would probably be found that in all such cases there was some change in its molecular arrangement, similar to that described in railway axles.—Mr. James Nasmyth confirmed Mr. Fairbairn's statement, as to the injurious effects of swaging or hammering iron when nearly cold; and suggested that the only safe course was to have all axles annealed after forging, by which the injurious effects of injudicious workmanship would be entirely got rid of.—The Chairman enquired whether Mr. Nasmyth supposed that annealing would remove the crystalline texture resulting from use, and restore the fibrous texture of the iron.—Mr. Nasmyth said he had no doubt it would have that effect. He then made some very interesting observations on the absence of oxidation from those iron rails of railways on which the traffic was only in one direction, and observed that on the Blackwall line, where the trains travelled backward and forward on the same line, the extent of oxidation was very striking.—After some further discussion, in which Sir John Robinson, Mr. Grantham, Mr. Lucas, Mr. Burdekin, and several other gentlemen took part, there was a pretty unanimous expression of opinion, that the subject was one well deserving investigation; and that the funds of the British Association could not be better employed than in defraying the expense of a series of experiments on a point so interesting to scientific men, and so important to the public; and Mr. Vignolles expressed a hope that next Monday would not be permitted to pass over without a motion for a grant for that purpose. At the conclusion of this discussion, the Chairman said he thought it would be quite useless to attempt to fix the attention of the section on any other subject, and he therefore proposed to adjourn to the following day; which was acceded to, and the section broke up.

THE FIRST GENERAL MEETING OF THE ASSOCIATION.

As was previously announced, this the first general assemblage of the members of the association at their twelfth meeting took place on Thursday evening last, in the spacious meeting-house of the Society of Friends, Mount-street, which, it is said, will contain about 2,000 persons. It was considerably more than half-filled, and a very great number of ladies were present. The assembly was indeed in every respect a most brilliant one, and the greatest interest appeared to prevail throughout the proceedings, notwithstanding the period of time which they occupied, and the comparatively late hour of the evening at which they commenced.

On the "preacher's gallery" were the presidents, retiring and elect, Roderick Impey Murchison, Esq., and Lord Francis Egerton, M.P., the Marquis of Northampton, Sir David Brewster, Sir John Herschel, Rev. Professor Whewell, General Sir Thomas Brisbane, Sir J. Robinson, Colonel Sabine, the Rev. Dr. Peacock, Dean of Ely; the Rev. Professor Powell, Professor Willis, Dr. Dalton, Dr. Turner, Sir H. T. de la Beche, the Hon. and Very Rev. Dr. Herbert, Dean of Manchester; the Rev. Richard Parkinson, B.D., Canon of Manchester; Colonel Sykes, the Rev. William Vernon Harcourt, Sir William Jardine, John Scott Russell, Esq.; Professor Sedgwick, Sir Charles Lemon, Dr. Richardson, Sir M. I. Brunel, Sir Oswald Mosley, Henry Hallam, Esq., F.R.S.; George William Wood, Esq., M.P.; Rev. Dr. Halley, Colonel Wemyss, Mr. Griffiths, Surveyor General of Ireland; Colonel Pringle, R.E.; George Webbe Hall, Esq.; R. D. Davenport, Esq., of Capesthorne; John Taylor, Esq., Treasurer of the Association; Professor Phillips, General Assistant Secretary; &c. The seats immediately in front of and below the gallery were reserved for the committee of the association.

At twenty minutes past eight o'clock, the retiring president of the association, the Rev. Professor Whewell, F.R.S., &c., took the chair, amidst great applause; and proceeded to open the business of the meeting. He said it was not necessary that he should announce to any one present, that this was the twelfth meeting of the British Association, or that this was the first day of that meeting; on which, according to the custom observed on former occasions, it was the rule that the president and the other officers of the association should appear before the assembled members. His position here was that of an individual who held the high office of president; but his business in that capacity was very brief: it was merely that of surrendering the cares and duties of his office into the hands of his successor. It might perhaps appear an anomaly to some, that he should continue, not only to act as chairman, after the appointment of his successor, but even after the business of the present meeting of the association had begun; those, however, who knew anything of the mode in which their proceedings had been conducted on former occasions would be aware of the circumstances which led to that arrangement. On the first day of their assembling, many members from a distance did not arrive early in the day, so that the general meeting could not be held till the evening; yet as it was not deemed advisable to lose a day in waiting for the arrival of all the members, the various sections were accustomed to meet for the transaction of business; and he had much pleasure in announcing that the whole of the various sections had already been most actively engaged, and that they had got through a great deal of business. His duty now, however, was very brief and simple. It was merely to hand to his successor the torch of knowledge; which you commit to us (said the chairman), which we hand to one another, and which derives all its powers of illumination

from your exertions and your talents. I have often thought, in seeing this office transmitted in this manner, from one to another, in having unexpectedly received it myself, and in now transmitting it to my successor, I might say, in the words of the poet—

"Et quasi cursores vital lampada tradunt,"

which I may alter to

"Et quasi cursores musarum lampada tradunt,"

which, if you will allow me to offer a translation, as ladies are present, I would say—

"As in the torch-light of the Grecian youth,
We pass from hand to hand the lamp of truth."—(Applause).

As he had now filled all the various offices of the association, it was impossible that he should not look with no small degree of interest to its future fortunes. It was impossible that he sould not feel some anxiety in glancing into its future, that he should not wish to know in what respect it might resemble or might differ from the past. During the period which had elapsed since the formation of the British Association, it had successively visited York, Oxford, Dublin, Cambridge, Edinburgh, Bristol, Liverpool, Newcastle, Birmingham, Glasgow, Plymouth—great cities, powerful and opulent, among the greatest in the empire—and it is now held in Manchester, inferior in these respects, we might say, to none. (Applause). In this manner, the British Association had gone through a great number of the most prominent and distinguished towns in the empire; and as long as the association came to such places, its members were sure to find persons who would share in and take an interest in their labours. An abundant number of persons would be found ready to engage in those laborious offices which were necessarily connected with the preparations for the reception of the association, and the rendering of their proceedings agreeable and useful. And when the association left this town for another, they would go with a feeling which he ventured to express, on dissolving the last meeting at Plymouth, "To-morrow to fresh fields, and pastures new." But the numbér of towns so circumstanced in the empire was limited; and if they looked forward to the time when such pastures began to fail — when there were no opulent cities able thus to receive us, which were not visited before, the association were naturally led to enquire what was the course it should then take. It appeared to him, that there were three courses open to them. After visiting all the great towns, the association might then go to towns of a smaller size, which might derive their power of receiving them not only from themselves, but from those districts which they represented; or the association might return to those towns in which they had already held their meetings; or it might be found convenient to suspend the proceedings of the association, and look forward to some future course. He wished it to be understood, that in making these observations, he spoke as an individual, and without the sanction of any official persons. He trusted that his views, thus

briefly expressed, would be accepted as an evidence of the strong feeling and anxiety he had with regard to the future prospects of the society. (Cheers). In following out their original intentions, of course the society would continue to visit new places as long as possible, and thus to make known to others the merits and labours of scientific men in the provinces, who had previously been in comparative obscurity. No doubt, there would be many difficulties in the way, but they would all be overcome; the modes of proceeding of the association were not so inflexible as not to admit of adaptions according to the circumstances of the place. With regard to their returning to places where they had already been, that course should be, of course, delayed as long as possible. Perhaps in this respect, an exception might be made of that city which might be looked upon as the mother of the society, and which might naturally feel an anxiety at no distant period again to see her offspring; after looking on it with all a parent's natural solicitude, it must have a desire to see her child, who had been so well received in every part of the empire, coming back full of life and vigour, and laden with honours to gladden her eyes. (Applause). With regard to the third course: the having the activity of the association suspended for a time, he saw no evil in it. This however was an event which would not arrive for many years; and the more remote the contingency was, the better satisfied should he and all the friends of the association be. (Cheers). The association had accomplished much of what it had, at its formation, proposed to itself: no one now could say that Englishman were not aware of the state of science on the continent, and had not done what they could to advance science in their own country. But that was no reason why they should not go eagerly on; and he for one should be ever ready to do what ever seemed most likely to carry them in the advance. (Hear). It was with peculiar gratification he found himself called back on such an occasion as this to his native county: called back to scenes within sight and reach of those who were endeared to him by all the ties of childhood: to be brought again in contact with friends some of them the most cherished he possessed, and in this way knitting up the ties of childhood in the maturer period of his life. He also came here with peculiar pleasure, because he met with a kind and valued friend of many years: a man who belonged not to one but to all of us, by ties of a far wider and more exalted kind. It was with peculiar pleasure and gratification they all found themselves gathered around that exalted, that great philosopher, who belonged peculiarly to Manchester, but whose name was the highest name in chemistry in every part of the globe. (Loud applause). He (Mr. Whewell) could not describe, but the meeting felt for themselves, the pleasure it gave him to see his venerated head appearing in our sectional rooms, bearing about him a halo of well merited honour and reputation as wide as the world. They felt in the veneration which they entertained, and which they knew the whole world entertained for him, a pledge of the dignity and the purity of that love of

science, and veneration for science which brought them together. (Cheers). Nothing now remained for him (Mr. Whewell) but to resign his chair to his successor; and he hoped he might be allowed to express his gratification he felt in resigning the sceptre with which he had been invested into the hands of one so accomplished, and so well versed in literature and art: one in whose occupation of the first seat of the British Association for the advancement of science they might see a recognition of the bond that binds together all branches of literature, and departments of human cultivation. (Cheers). He hoped he might be excused for recalling to the recollection of his successor a portion of his classical studies :—

> "Me vero primum dulcis ante omnia Musæ,
> Quarum sacra fero ingenti joculans amore
> Accipiant cœlique vias, et sidera monstrent;"

which I may translate thus :—

> "Him who has loved the muses well and long,
> And won their smiles in fields of art and song,—
> Him will they welcome in that skyey zone,
> Where stars and worlds no less their empire own."

The Rev. Professor sat down amidst loud applause.

Lord Francis Egerton, M.P., then, amidst considerable applause, took the chair, and said that the first duty he had to perform was to call upon the general treasurer for his report.

John Taylor, Esq. the treasurer of the association, stated, that the balance in hand from last year's accounts was £367 3s. 10d.; life compositions and annual subscriptions at the meeting at Plymouth, £1,131 1s.; ladies' tickets there, £261; compositions from members for the delivery of future volumes of the reports, £513 2s.; half-year's dividend on £6,000 in three per cent. consols, £90; proceeds of the sale of £500 of that stock, £452 1s.; for reports sold £89 3s.; making a total of receipts of £2,903 10s. 11d. The payments were—for the expenses of the Plymouth meeting, and sundry disbursements by the general treasurer and local treasurer, £321 16s. 3d.; paid for printing and engraving for the volumes of reports, £288 3s. 4d.; salaries of the assistant general secretary and the accountant, £305. The total of a long list of payments to the scientific grants, £1,449 17s. 8d.; balance in the hands of the bankers and local treasurer, £538 14s. 6d.: that was the present state of the accounts. The property of the association was that balance of £538; £5,500 in the three per cents. valued at £5,018; and the value of the stock of books on hand, estimated at £1,130; making the whole property of the association, £6,687 9s. 6d. Since their arrival in Manchester, 771 tickets have been taken by new and old annual members, for which £1,396 had been received, and £265 for ladies' tickets (£1 each); making the total amount received in Manchester, £1,661. (Applause). The chairman then called on the assistant general secretary for his report.

Professor Phillips, the assistant general secretary, then made his report, with respect to the general arrangement of the meeting.

As to the working of the body, as regarded the advancement and promotion of science, there would be no material change; it had been gradually improved, and at present, within the limited period of six working days, it did not appear capable of any considerable change for the better. This was a place where great exhibitions of mechanical ingenuity were produced; and those who saw the two classes of exhibitions here would be much delighted. Whole collections had been moved—whole societies had been thrown into disarrangement—to make preparations for the meeting of the association. Individuals had contributed their own valuable cabinets of natural history to enrich the valuable stores of the Manchester Geological Society. After referring to the mechanical exhibition, to the admissions, to manufacturing, and the excursions, in much the same terms as already reported in his explanatory address at the general meeting of the committee on Wednesday, he said that the Sheffield Railway Company, had made an offer to the association to inspect the great tunnel now making, after the meeting. Another excursion also, after the meeting, was to those subterranean collieries and unparalleled tunnels at Worsley, which the noble president had given an opportunity of inspecting. (Applause). On this occasion there had been great success in the preliminary arrangements for this meeting. If there should be a material failure in any part of the arrangements for this meeting, he should be greatly distressed; but they were all greatly indebted to the local secretaries and officers, whose arrangements had given such satisfaction in the sections to-day to all the members. It would be a painful feeling on the part of many persons, if they thought they were not to visit again more than once, indeed many times, the places where they had derived so much gratification at these meetings. (Applause). He expressed his gratification to see the head of Dr. Dalton stamped upon the cards of membership for this meeting. (Applause).

Lord Francis Egerton, M.P., then rose, and said, Gentlemen, as your late president has informed you, eleven years have passed since the great prototype of this meeting was held at York; and such was its success, that, as you know, the experiment has been annually repeated ever since, and with similar and augmenting results. His lordship then referred to the practice of the president of the year giving on these occasions a brief, but instructive, retrospection of the state of science as connected with the past and contemplated proceedings of the association; which, he said, was however, inconsistent with its other practice of admitting to the temporary honour of its president an individual like himself, selected not for any scientific pretensions, but solely from the accident of local connection with the place, rather than with the objects of the association. (Applause). I cannot forget (continued his lordship)—I wish you could—under what auspices the last meeting in Plymouth was held; I cannot be unconscious of the fact from whom I have on this occasion received the seat which I have now the honour to fill. Could it be forgotten, it were hardly to my interest to awaken the recollec-

tion of the fact, that Professor Whewell filled at Plymouth, last year, the situation which I have the presumption to fill at Manchester. If I do so, it is only for the purpose of observing, that if he, who has "run through each mode of the lyre," and proved himself to be " master of all," should express his sense of the difficulty of endeavouring to convey to a mixed audience, within the limited time allowed him, such a summary view of the state of science, it would not be for me to make an apology for not imitating his example, but rather to call upon the council to give reasons for not calling upon some other functionary of the association immediately to execute that purpose for which I am so utterly inadequate. Some observations, indeed, before I sit down, I may allow myself, which I consider illustrative of the advantages of the society, and of the reasons which have impelled me, and many others similarly situated to myself, to give whatever feeble influence we can to its proceedings. But before I proceed to such topics, allow me to indulge for a moment in the expression of my feelings of satisfaction upon the subject of the locality which sees us assembled on this occasion. On this subject strangers and guests will excuse me, inhabitants will sympathise with me, if I express some feelings of complacency upon that topic. (Applause). It not merely that the place which sees us here together, has from various causes attracted to itself, as to one of the principle centres of the world, so vast an amount of mechanical skill and invention: it is not merely that a neighbourhood so rich in mechanical treasures offers in itself attractions to the followers of many most important branches of natural science;—there is another reason, equally weighty, I think, and upon which I dwell with even greater satisfaction now. It is because this town is the birth-place, and is still the residence, of one whose name is mentioned with the greatest respect in whatever part of the civilised world knowledge is cultivated;—(applause)—one whom I am happy to see here to-night near me, to enjoy the honours which he has won by a life of persevering exertions in the cause of knowledge; and I beg him to accept from myself, if he will condescend to do so, the expression of my most sincere regret (and no one here can feel it more than I do), that the increase of years, which to him has been but the increase of wisdom, should make him, with reference to his physical strength, reluctant to fill an office which in this case would receive more honour than it could confer. (Applause). I do regret that from this, or from any other cause, such an assemblage as this within his native town, should miss the opportunity of being associated with the honoured name of Dalton as its president. The council well know my own views and feelings on this matter, and that if my humble services could have been available, I would gladly have served as a doorkeeper in any house in which the father of science in Manchester was holding the office of president. (Applause). I must offer this apology for my occupying this situation, that I may at least do no prejudice to the cause which we are met to support. To those who have originated this

institution, who have tended it from birth, who have watched it from its cradle at York to its vigorous maturity at Manchester, who manage its affairs and regulate its proceedings, and who have called upon me to occupy this chair, I respectfully leave the task of my vindication. In addressing you upon any topic connected with this society, I can only do so in one manner. All readers of German literature and works of science, cannot have failed to notice the frequent recurrence of the word *steinkee,* which signifies the place from which the speaker or writer views the object which he is discussing. My position, in reference to this association is dim, indistinct, and shadowy. I am not even a proselyte of the gate, far less a Levite or a priest of the sanctuary; my lips cannot pronounce the shibboleth of the temple of science; and though I would fain worship at a distance, yet the sound of the ritual falls too faintly on my ear to allow me to join in the service of the altar. Yet I can approach the the edifice near enough to knôw that the architects are busy, that the builder is at work; I hear with you the clink of the hammer and the trowel. The pile is a vast one, but what man shall ever call that pile complete? Many a shaft remains yet to be polished, and many a capital to be elaborated into new forms of fitness and beauty. The architects are now busy on that ground where Bacon shaped the rugged top of that Moriah of philosophers, and smoothed the way: removing the rubbish of centuries, and shaping it into a vast, splendid, and solid basis, for the subsequent discoveries of Newton and his followers. (Applause). I hear the sound of their labours; but it is not for me to attempt to instruct you in that of which I am ignorant: the progress or the details of their labours. These are points which you will learn in those sectional departments into which the builders have wisely divided themselves. There the electrical inquirer will be enabled to learn into what new shapes and channels his fellows are directing that subtle fluid which Franklin snatched from heaven; and what forms they have compelled that Proteus to assume whom they have enslaved to do their bidding. Mr. Lyell, I believe, is still pursuing his investigations in the remoter regions of the new world; and my friend Mr. Murchison, has returned rich in treasures from his travels and researches in an important part of the old: and returned to tell you of the favour he received from the sovereign of those vast dominions. (Applause). With the power, the schemes for conquest, or the military projects of that monarch, or any other sovereign, we have nothing to do, and our thanks are justly due to him for the homage he has thus rendered to science. "*Quid bellicosus cantabes aut scythus agat?*" is no topic for us; but it is a topic for the just acknowledgment of our gratitude that our friend Mr. Murchison should have received from the sovereign of those vast realms the reception by which he was entertained, and which stamps that sovereign as the friend of science. (Applause). The communication and discussion of such past achievements and researches as these is one of the useful and legitimate objects for the operations of the society. Fortunately, we are now arrived at a pe-

riod of the society's operation, when it is not incumbent to show something of the probability of prospective advantage from its continuance. It might be well for those who originated its operations to make out their calculations and estimates, as you do in one of those schemes for carrying a new railroad through the country; but we have at least arrived at a period when we are able to show, not calculations and estimates, but profits and dividends. (Applause). It was easy to foresee and foreshow, from the opportunities for mutual discussion between persons resident in different parts of this country, and in different countries of the globe, that from the collision of such minds light and heat must ensue. It was easy to predict, that from the nomadic principle of this society (if I may use such a term), the light of science would be carried in its brightest and purest form into those parts of the country where it has hitherto shone with comparative faintness. All this was easy to predict; and, fortunately, it is not difficult to show, that those predictions have been more than accomplished in many most important points. It was observed last year, by my predecessor in the chair, and I believe it has been remarked at meetings on former occasions, that up to a recent period, of all the main branches of natural science, astronomy was the only one which had received the direct and permanent assistance of governments, and, if I may use the expression, had enjoyed in general the patronage of society at large. It was well that precedence should be accorded to that most sublime and most ancient branch of natural science; and there are other reasons, which in this maritime country, most undoubtedly recommend it to that especial patronage of government. Of astronomy we may say, gentlemen, that it was well she should walk first, but not that she should walk alone. There are many other branches of enquiry which stand much and equally in need of the assistance of the state, of combined operations on the part of individuals, and of assistance in respect of pecuniary support. Now, the details are quite beyond my province to state to the meeting; but it might be most satisfactorily shown to any assembly, that in these respects this association has been of the greatest service. There are many subjects on which its advice had been received, and followed; many objects for the promotion of which the assistance of its funds had been accepted; and on these points it may be most satisfactorily shown that this association has been of the greatest direct and practical service to the cause of science in this country. (Applause). Astronomy, I have said, took precedence of other sciences, especially in the favour of governments and nations; and undoubtedly now the connection between astronomy and the government, between Greenwich and Downing-street, is founded on the most solid foundation; but it has not been always that astronomy has so won her way to favour in the courts and councils of princes. I believe she once owed that favour to the respect then entertained for the claims of judicial astrology.

But astronomers do not now point their telescopes, as Wallenstein did, to the heavenly bodies, in order to read from them the mysterious future. The English soldier knows but one Homeric omen —that the defence of his country is the performance of his duty. (Applause). Some two centuries ago, and I believe Mr. Airy might have been distracted from his more important investigations and calculations, to mark what star was culminating on such occasions as the birth of a royal infant. We do not now watch the configurations of the planets on such events; but to that Providence which has shielded the mother (loud and long applause) and to the prayers unto that Providence of a loyal people, we cheerfully confide the fortunes of the infant hope of England. (Applause). The sun of science has drunk up all those delusions; but, as I have said, substantial grounds still remain, why that connection between science and the state should be powerfully exemplified in the case of astronomical pursuits. Even here, this society has not been wanting in its assistance. I believe that no scientific labour of more importance (as I am informed) has been suggested or exercised, than the reduction of the observations at Greenwich, which has been going on at the expense of this society; unless it be that equally important, which has been suggested to government, to support the reduction at its own expense: following therein the example and the suggestion of this institution. (Applause). Upon this subject, if I needed any confirmation, I believe at a subsequent period of this meeting I might enjoy the opportunity of appealing to the greatest authority on such subjects that continental Europe can produce; for I find the authority of Professor Bessel (who is not here yet, but who is expected), whose opinion on the records of this society is expressed in the strongest manner with respect to those very observations of which I speak. Should that eminent individual arrive here, as I understand is expected, in company with Sir John Herschel, on Monday, it may be said few railroads have had a more important charge than the London and Birmingham Railway will have on that occasion. It is an old saying of Adam Smith's, that of all luggage man is the most difficult to transport." (Laughter). It is very fortunate that the difficulty is not commensurate with the value of the article; for if it were, whatever power of invention and mechanical skill my friends Sharp and Roberts may possess, I doubt if they could construct a locomotive that could drag those two eminent philosophers to Manchester. (Laughter). You are well aware, that of Sir John Herschel it does not become me to say one word in any British assembly; of Professor Bessel, you are well aware, that he by all the astronomers of Europe, is said lately to have achieved one of the greatest triumphs of astronomical science: the accuracy of whose observation, and the grasp of whose calculations, enable him to overleap the bounds of our visible celestial system and the orbit of Uranus, and to calculate the parallaxes and distances of some, at least, of those remoter bodies, whose distance mocks our powers of contrivance to magnify their bulk to our

vision. (Applause). I have only to express my regret that it is not in my power to give him that welcome that I am sure the meeting would bestow upon the presence of so eminent a man. (Applause). The connection between science and the powers of the state is a matter of more importance and difficulty than I can enter into on this occasion, but I think I can show additional proof, besides the mere brief reference to past transactions, to that magnetic expedition which is now proceeding, originally at the suggestion of this society and at the expense of government; and beyond the fact of this society's operations, particularly the survey of the kingdom now proceeding, which has been, as to this part of the kingdom, extended in its scale, almost entirely in consequence of the movement and impulse given to the government in that transaction by the same body. But I allude to a more recent instance: to show, that the connection has been established in a striking manner, and upon what I think a sound footing. I wish to see science connected with government, not in any low or dependent form; not under the undue control of government; not dangling in ante-chambers, or sweeping the dust from the floors of public offices or palaces; but seeking, receiving, and requiting with usury, the occasional assistance of government; enjoying a liberal degree of favour and good will from the powers of the state. It is known, I believe, to most of you, that recently a building which has been left useless, which was formerly appropriated to the purposes of science, was at the disposal of the Crown. A suggestion emanated from this society, that it might be of service—of far more important service than is even now contemplated—that it was a building which might serve as a situation where your instruments may be preserved and compared, and for various other uses applicable to various branches of science. I am happy to say that the sceptre was promptly and graciously extended towards us, and that the observatory at Kew is now at the disposal, and will shortly be at the use, of the body which I have now the honour to address. (Applause). On looking through the transactions for the year 1839, I was struck by a passage which seemed to be very illustrative of the practical effect of your proceedings; for, in the preliminary passage of Professor Owen's treatise on the fossil reptiles of this country, he distinctly states, that but for the co-operation and assistance of this society, it would be impossible for one man to have embarked in that subtle and laborious task, which he had since so ably executed and performed. I ask you to look upon the pages which form the commentary on that text. It is a subject which unlearned men like myself may all partially bring within their comprehension; it does not invole those trains of algebraic formulæ which puzzle the uninitiated, or those symbols which, to such as me, are nothing more than hieroglyphics;—you will there follow Professor Owen through the relics of former worlds; you will see how he marches, with order and arrangement in his train; how the dislocated vertebræ fall into their places; how the giants of former days assume their due bulk and dimensions—some of them shorn, perhaps, of the proportions

which on their first discovery, were attributed to them, and some enlarged;—peruse that work, which tells you that it owes its existence to the encouragement given by this association; and I say, that on the pages of your own transactions you have proof enough, that the operations of this society have not been ineffectual or useless. (Applause). Before I sit down, I would endeavour to illustrate my feelings by reference to another scientific transaction. About two years since, an adventurous party, of which Professor Agassiz was at the head, achieved the ascent of those Alpine heights, which, as its very name implies, had for ages been supposed inaccessible to the foot of man. It is probable, that there were many who from the *chalets* and the pasturages below, directed their telescopes to those peaks of ice, with the warmest interest for the safety and the success of those adventurers. Perhaps there were some who, by trifling excursions into those regions, had learned to understand and know the difficulties of progress in those higher Alps: who knew something of the dangerous crevices: who could tell of the ascent, cut step by step with hatchets in that precipice of ice, and who could appreciate the adventurous magnitude of the enterprise. Be assured, you climbers of the heights of science—and there are many of you here—that there are those below who sympathise with the efforts which they cannot share or emulate; who rejoice in your success, who lament when you are baffled; and, when you plant your flag upon some hitherto virgin summit, their shout of applause would reach you from below, if it could be conveyed to your organs by the pure and attenuated atmosphere which it is yours alone to breathe. (Loud applause). Dwellers in a dull valley as we are—breathers of a heavier and too oft a tainted atmosphere—we can yet look upward. We count your triumphs, and, as you gain them we gladly place yonr names on the list of the recorded benefactors of mankind—(applause); for it is the privilege of triumphs like yours, that though they become common property, though they extend advantages wherever civilisation extends over the habitable world, yet at the same time, and for that very reason, they exalt the country from which they originate in the scale of nations, and fulfil the most rational feelings of national pride, while they perform the obligations of our common humanity to the most unrestricted extent.—His lordship resumed his seat amidst loud plaudits from every part of the assembly.

Mr. Murchison: As an old soldier of the British Association, it is my duty to state, that, in compliance with the request of the officers of the association, that the noble lord would undertake the office of president, Lord Francis Egerton is the first individual, who occupying such a high place in public estimation, and of great local influence, who has attempted that task which has been heretofore confined to the officers of the association. (Applause). How much we (I mean particularly Lieut. Col. Sabine and myself) are indebted to the noble lord, has been amply testified. I have always felt, that during the progress of the association a time might arise, when

some one combining in his own person scientific and literary attainments—scientific I say, though the noble lord does not choose to acknowledge acquirements of this sort—united to public station, might become to be the fittest person to preside over us. How truly this anticipation has been verified, the success of the meeting has amply shown. (Applause). The statements which you have just heard from the noble lord must have a great effect on the country. Coming from such an authority, they must have greater influence than from any humble scientific individual. After the advice we have just heard from the noble lord, I think we are bound to express our gratitude for his speech. I move, therefore, that the thanks of the association be given to Lord Francis Egerton for the able and enlightened discourse he has this day given us. (Prolonged cheering).

The Marquis of Northampton: I rise with great pleasure to second that motion. It does not require one word from me to recommend it; but, having attended a great many former meetings of the association, I may be allowed to express the great satisfaction with which I have listened to the speech of my noble friend to night. (Applause). My noble friend has alluded to classical authorities; and I may say, that as long as we have presidents like him who has just addressed us, if ever it should be the fate of the association to die, our memory will be *cara fatæ sacro*. But I am certain that the British nation will never allow the British Association to die: nay, that they will not allow its existence to be suspended even for one year. (Applause). I agree with your late president, that as long as fresh " fields and pastures new " invite us, it is our duty to go to them, with due consideration of the convenience of the association. And whenever the time comes that we have completed our cycle, I am sure there is no place that has received us before, that would not be glad to receive us again. (Applause). I have said this at former meetings of the association, and it has always been responded to as you have responded to it. It is now five years since the association was requested to come to Manchester. Your venerable fellow citizen (Dr. Dalton), the father of science, not only in Manchester, but in the kingdom, came five years ago to Bristol to urge us to come to Manchester. It is only candid to say, that I opposed your claims—(hear)—because I thought it better at the time that we should go to Liverpool. The invitation was repeated again and again, and here we are. And now I anticipate, that that application will be again and again repeated until we come here a second time. (Applause). I don't say, for the purpose of enlisting your assistance and co-operation in the great work in which we are engaged, that you are thereby conferring a benefit on humanity; but I say to you, who depend on commerce, on manufacture, and on your mechanical skill, that the cultivation of science is of the very utmost importance to your existence; and that it is only from following up inquiries, and pursuing studies of this description, that your town, as well as other towns of this empire, will continue to flourish.

If, however, England allows other nations to get the start of us, while we are slumbering in our beds, Manchester and other towns like it must be destined to utter ruin. (Loud applause). My noble friend has mentioned the extended, I may say gigantic, inquiry now going on in different parts of the world in the shape of magnetic observations. I believe there are at this moment going on, in not less than forty observatories—[A member: "Fifty-one"]—in different parts of the world, a portion belonging to this country, and the rest to other countries. I have the satisfaction to inform you that her Majesty's government, at the request of the Royal Society—they would have, I doubt not, granted it to your request, if the Royal Society had not forestalled you—have consented to continue these magnetic inquiries three years more. (Applause). Another subject connected with this has been adverted to: the kindness shown to science by the Emperor of Russia. I may state upon this, that when I waited upon Sir Robert Peel, on the part of the Royal Society, my companion was the Ambassador of Russia. (Applause). Having taken an active part in furthering these inquiries, the emperor desired his ambassador in this country to make these representations in favour of continuing them. I thought it best that we should go together, and thus exemplify the important truth, that science is a bond of union among all nations, and the best promoter of peace and amity. (Applause). I can't help saying again, although I have said it before, that it is important to the progress of civilization, of humanity, and peace, that nations should feel a common interest in pursuing together those scientific inquiries in which they have a great common object. (Applause). It has been suggested that we go to York next year; I am sure we shall return with as great satisfaction as a child goes back to its mother. At the same time I am bound to remark, as it is well to have everything above board, that the determination of the places at which the association holds its meetings is reserved to a particular body in the association, and that it is impossible to resolve you, ladies and gentlemen, into a parliament, for the control of the local details and general principles which govern the association in its determination. (Hear, hear). I will only express a sincere hope, in conclusion, that the association will continue as eternal as the truths it is designed to discover.

The motion was carried with loud applause. The noble marquis concluded by seconding the vote of thanks to the president.

The President: It is now my duty to inform you that this meeting is adjourned to Wednesday evening next, at the same hour.

The proceedings terminated at a quarter to ten o'clock.

FRIDAY.

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCES.

The Very Reverend George Peacock, Dean of Ely, took the chair at a quarter past eleven o'clock.

Professor Stevelly read a report by Mr. Francis Baily (assistant

astronomer royal), "On the reduction of the stars in the *Histoire Celeste*" by Lalande, the whole of which, with a few omissions, have been reduced; being in number upwards of 47,000 stars. The cost of printing a catalogue of these stars, in octavo, 500 copies, about £415; or 1000 copies, £100 additional; or with expenses of arranging for the press and correcting, £500 for 500, or £600 for a 1000 copies. Should the British Association decide upon printing, Mr. Baily would draw up a statement of the mode of making the reductions.—The Chairman said a grant of £500 had been made for this purpose, several years ago, as the immense treasury of astronomical knowledge published in the *Histoire Celeste*, by Lalande, was absolutely useless for the purposes of astronomy without such reduction.— Mr. Stevelly then read another report from Mr. F. Baily, "On the British Association Catalogue of Stars," in which the calculations of the proper places of the stars, with the logarithms of the proper constants, &c., were furnished for nearly 83,000 stars. The whole were copied out for the press; the expense of printing 500 quarto copies would be £550; 1000 copies would be £150 more.—The Chairman explained that this labour had been undertaken under a grant for the extension of the catalogue of stars of the Royal Astronomical Society, which was published by subscription of the members of that society many years ago; and this new catalogue was much needed. It was certainly one of the most important of the labours undertaken under the auspices of the British Association, and its publication would certainly be one of the greatest contributions for the service of practical astronomers that could be imagined. It was proposed to name it when published, "The Catalogue of Stars of the British Association."—Mr. Stevelly also read a report by Sir John Herschel, on the reduction of Lacaille's stars, made by a committee consisting of the reporter, Mr. Henderson, and Mr. Airy; and under the superintendence of Mr. Henderson, the whole of that work was now completed; and the resulting catalogue, arranged in the order of right ascension, was fairly written out for the press. The number of stars reduced was about 10,000. Sir John Herschel recommended its publication, without which, little or no benefit could result to astronomical science. The introduction to the catalogue would give an account of the process pursued in the reduction, the constants used, and all explanations necessary for understanding the work.—The Chairman said this great work had also been in progress several years. The observations of Lacaille had latterly become of great importance, inasmuch as they were made at the Cape of Good Hope, and comprehend a very large class of equatorial stars, as there was now an observatory, &c., established at the Cape, with regular observations made and reduced; and they were also valuable with reference to the labours of Sir John Herschel in that locality, in order that we might have the observations of two distant epochs to compare with one another.

Colonel Sabine then made a report from the committee for trans-

lating and publishing foreign memoirs. In the past year two memoirs had been selected for publication, which had been translated gratuitously and presented to the committee by a gentleman who was not a member of the association, and they had appeared in the 10th number of *Taylor's Foreign Scientific Memoirs*. In answer to a question of Professor Stevelly, Colonel Sabine explained that the translation of one of these memoirs, by Professor Dobee, of Berlin, on the law of storms, would enable any person (a mere English reader) to use his valuable tables (perhaps the most valuable digest ever made for the science of meteorology), as profitably as if his book were translated.

Sir David Brewster then made a communication "on a new neutral point and a secondary neutral point in the atmosphere." After noticing the two neutral points (points where there was no polarization of light) of M. M. Arago and Babinet, Sir David Brewster said he had discovered a third at a distance of 20° below the sun. He also mentioned amongst some general results of observations continued for a long time, that instead of the point of maximum polarization being always, as supposed, 90° from the sun, he had found it more frequently 88° from the sun. The neutral point of M. Arago existed in the horizon all day between November 17 and January 24, the altitude of the sun being then such that it just touched the horizon during that time. It was generally believed that the polarization, at the maximum of 90° from the sun was produced at an angle of 45°, but he found the angle only 30°,—i. e. that the motion of the plane of polarization was equal to 30°, and might be obtained by the following formula:—$Cot\ \phi = Cos\ (i—i')$. He found that when the polarization was extremely weak, there was always a quantity of white vapour floating in the atmosphere. Another result of his observations was opposite to the one given by M. Arago, who thought that the curves of the equal polarization were concave towards the sun, whereas these observations showed that they were always convex towards the sun. Sir D. Brewster then adverted to the secondary neutral point of polarization which he had discovered in peculiar states of the atmosphere. He also described a polarimeter, or polariscope, formed of several plates of glass, two plates of rock crystal cut in a peculiar way, and a plate of tourmaline; by which, he said the rectilinear bands in polarization were seen more clearly than by other methods.

Professor Baden Powell made a communication "on certain cases of elliptic polarization." The only substance previously known not metallic, in which elliptic polarization existed, was mica, in a peculiar form, as stated by Professor Forbes at the meeting of the association at Birmingham; but he (Professor Powell) found elliptic polarization existed in many substances, amongst others plumbago, which was metallic only in a small degree. He then showed a coloured representation of the polarization observed on some steel plates, produced by Nobili's process, by a galvanic deposit upon the

steel plate, which exhibited singular phenomena both in form and colour, there being a regular order of colours from yellow to red, deep purple, blue, green; and then began a second order of yellow, &c. The whole proved experimentally what Dr. Lloyd had predicted theoretically, at the last year's meeting of the association, in reference to experiments of Sir David Brewster on thin films.

Mr. J. Scott Russell then made a supplemental report of the committee on waves. This oral communication related to a third inferior class of waves, to which Mr. Russell gives the name of capillary waves, as resulting from the same causes which gave rise to the forces of capillary attraction. If he inserted a small wire or glass rod, a sixteenth of an inch in diameter, into a fluid in a state of repose, that fluid was raised by the capillary forces. He then described what he called the constrained motion of the wave, thus caused; and also the free motion of waves generated by the insertion of a point into the surface only of the fluid, or by the removal of such point from the surface. He had also observed the velocity and length of the common wave, which might be called, perhaps, the Newtonian wave; which he ascertained by measuring twenty or thirty of them, all perfectly equal to each other. The result accorded accurately with the Newtonian law, inasmuch as the velocity varied precisely as the square root of the breadth, without the least relation to the height of the wave: but he did not find the absolute velocity assigned by the Newtonian law to be correct. For example, by the Newtonian law, the wave ought to be of the length, as measured by a seconds pendulum, of 3·2608 feet, and it was of the length of 3·57 feet. He then gave the following lengths of waves, in feet, and the respective velocity of each, in feet, per second of time:—

Length.	*Velocity.*	*Length.*	*Velocity.*
2· 65	3·01	3·913	3·72
2· 94	3·16	4· 20	3·84
3·125	3·29	5· 00	4·16
3· 26	3·37	6· 25	4·62
3· 57	3·57		

He gave these observations to show that the limits between them might be considered as perfectly correct. Thus a wave 3·57 feet long, always had a velocity of 3·57 feet per second, and this could never be confounded with the observation either preceding or succeeding it.—A short conversation ensued; and as the next paper in order, that of Professor Stephano Marianini, was in Italian, and the translation not yet completed, and the two next on the paper could not be read, as the author, Mr. A. J. Parsey, was not present, the proceedings closed here for the day.—The Chairman announced that two communications would be made to the section the next morning, from scientific foreigners; one by Professor Braschman, of Moscow, containing considerations on the doctrines of

equilibrium and motion, and another by Professor Jacobi, of Königsberg, also upon a mathematical subject; and he might say that no name amongst mathematicians of the present day stood higher than that of Jacobi. (Applause). The section then adjourned, at a quarter past one o'clock.

SECTION B.—CHEMISTRY AND MINERALOGY.

At the meeting of this section the Marquis of Northampton took the chair. There were also present, Dr. Dalton, Professor Graham, Dr. Daubeny, Dr. Plairfair, the Rev. William Vernon Harcourt, Dr. Apjohn, &c.

The first paper was a communication by Professor Haidinger, of Vienna, and was read by Mr. Croft. The subject was the Mineralogical and Geological Museum of the Imperial Mining department of Vienna, which Professor Haidinger was entrusted by the Emperor of Austria to arrange. The method of arrangement followed was the brilliant and original method of Professor Moh, which at once arrests the attention and challenges the admiration of the visitors of the imperial cabinet. Professor Haidinger described the number and contents of the rooms, and the order and arrangements, which were unparalleled by any other in Europe. The plan pursued was to divide the empire into four great general divisions, having the great rivers in the centre, and the principal chain of mountains as the boundaries on either side. A sort of section was represented by the arrangement of the cabinets in the four rooms, which greatly assisted the memory. The upper part of the cabinets contained the mineralogical and geological specimens from the higher ridges of rocks, while the tables in the centre held those from the plains. The collection altogether conveyed an idea of the geological and mineralogical peculiarities of that great empire from which it was obtained. It was not the actual possession of fossils and specimens that was useful, the professor remarked, but the use to which they were put, and to which an orderly and accurate arrangement of them mainly contributed.

The Marquis of Northampton expressed the pleasure which the account of Professor Haidinger's instructive manner of arrangement had given him. To obtain accounts of the method of arranging the museums of other countries was highly useful, since it materially instructed us in the arrangement of our own. One great difficulty was, at the same time to protect specimens and to enable them to be seen. He would suggest that the professor's paper be sent to the Geological Section. (Applause). The secretary (Dr. Lyon Playfair) then read a letter from Professor Haidinger, expressing the pleasure he felt at receiving an invitation to be present at the meeting of "that grand and beneficial institution, the British Association;" and his wish that his engagements would allow him to be present at the next meeting, where he might meet together, with other philosophers, his old friends Dr. Henry and Professor Sedg-

wick, whom he had known while partaking the hospitality of Mr. Allan, of Edinburgh.—The Marquis of Northampton remarked that Professor Haidinger had resided a long time in this country, and that he had, no doubt, added greatly to his geological knowledge while here. He (the noble marquis) hoped before long we should see in the British Museum a collection of minerals having especial reference to Great Britain.

Dr. Playfair proceeded to read a paper " On some new Oxides of certain of the metals of the Magnesian family." Chemists boasted of their acquaintance with the magnesian families, and yet our knowledge of them was at best very defective. What a vast hiatus was there not in the knowledge of oxides alone! The oxides of copper had a striking analogy to the oxides of magnesia. Dr. Playfair then proceeded to show in what manner he had obtained peroxides of copper, of iron, and of alumina. He was assisted by two diagrams, one showing the oxides of the magnesian metals, as far as we know them, the other showing the hydrates of peroxide of copper and of cupric oxide, with their cuprous analogues. These were given with their empirical and rational formulæ contrasted. In the course of his extempore address, Dr. Playfair expressed his opinion that the atomic weight of all unmagnesian metals might be doubled. —The vice president said the chemists present could not of course be expected to acquiesce in the new and curious statements respecting these peroxides, until they saw them proved by the analytic method, which Dr. Playfair could not adopt in a lecture. He had proposed some time ago that the atomic weight, for various reasons, ought to be doubled. He begged to express the great gratification he had derived from the statements of Dr. Playfair.—Dr. Daubeny expressed a similar feeling. Dr. Playfair had evinced great and minute chemical research, and had arrived at important general results.—Dr. Apjohn said a few words to the same effect.

Mr. John Mercer's paper " On some peculiar instances of Catalytic Action," was then read by Dr. Playfair by the request of the author, who was present. Catalysis acts with a power so latent as seems to defy detection, yet Mr. Mercer explained on ordinary chemical principles some effects hitherto described as catalysis. The author had arrived by a wholly different process at results strikingly similar to those of Professor Playfair in the last lecture.

The vice president said, Mr. Mercer was well known in Lancashire for his practical application of chemistry to the useful arts, and particularly to calico printing. He had now shown a not less intimate acquaintance with chemical theory, and had established himself a chemical philosopher of high excellence. His explanation of catalysis was in his (the vice president's) opinion, better than that of Liebig.—Dr. Playfair expressed himself of a different opinion on this latter point, but being referred by Mr. Mercer to the paper, and desired to read the passage again, he concurred with the vice president.—The vice president said that Berzelius had invented the

term catalysis to express a power that he could not detect. There was reason to believe that as these effects were from time to time explained on ordinary chemical doctrines, the term catalysis would disappear.—Mr. Davies said the papers of Dr. Playfair and Mr. Mercer possessed an extraordinary value and interest, and he begged to move a vote of thanks to both gentlemen.—Dr. Playfair on behalf of himself and Mr. Mercer declined the proposed compliment. Both himself and Mr. Mercer were members, and the association could not thank its own members.

A paper was then read, communicated by Mr. R. Hunt, of Cornwall, entitled "Researches on the Influence of Light on the Germination of Seeds and the Growth of Plants." The subject had been entrusted to Mr. Hunt for experiment by the association. He had provided six boxes, so constructed that no light could enter except through glass of different colours: the first being deep red, the last deep green. In these boxes he had raised ranunculuses, tulips, and other plants. The tulips he found germinated the first under the orange glasses, and last under the blue and green. Under the blue glass the plants, although slower in germination, were more healthy, and promised to come to maturity, and be perfect flowers; while under the orange they were more forward but sickly. A curious result was noticed with respect to the red glass. Under all other circumstances plants bent towards the light, but those under the red glass bent away from the light. In nearly all cases germination had been prevented by the absorptive power of the yellow rays.

Dr. Daubeny, as one of the committee appointed to investigate the subject with Mr. Hunt, hoped the committee would continue the grant to the latter gentleman. The results at present obtained seemed indecisive; and without wishing to throw any discredit on the experiments, he thought Mr. Hunt ought to have a further opportunity of establishing his principle, that chemical rays produced a specific and positive influence on the germination of plants.

It being now two o'clock, the vice president adjourned the section to the following morning at eleven.

SECTION C.—GEOLOGY AND PHYSICAL GEOGRAPHY.

The president, R. I. Murchison, Esq., took the chair this morning shortly after eleven o'clock, and opened the proceedings by giving a brief verbal summary of a memoir communicated by Dr. Dale Owen, on the Western States of North America. Two large panoramic views of the geological structure of that country were hung up in view of the meeting, with well executed diagrams of the various fossil remains hitherto discovered in that region; distinguishing the strata from which they were taken. One of these views represented the various geological formations observed in that part of the country, extending from the Wisconsin river, and passing along the

Mississippi, Illinois, Sargamon, Kaskasia, Embaras, Wabash, White, Ohio, Green, Cumberland and Tennesse rivers, to the Allegany mountains. The other views commenced at Chickshawbluffs, Tennesse, and proceeded across the state of Kentucky, in a course nearly north east, through part of Ohio and Pittsburgh, to the Allegany mountains. The president said, he had only just read the memoir, and therefore, his account of its contents would be necessarily imperfect. The memoir in detail, with a collection of fossils, sent by Dr. D. Owen, would be submitted to the Geological Society in London. The author was a native of England, who studied in the university of London, under Dr. Turner, and afterwards went to America. The country to which the paper referred, embraced Illinois, Indiana, a portion of Kentucky and Ohio. In this large portion of country, there were two coal fields, one of which (the Illinois coal field) was nearly as large as Great Britain. This great tract of country was traversed at its southern termination by the river Ohio. The other coal field was called the coal field of the Ohio, and was also very extensive. These coal fields were separated from each other by an axis of much more ancient rocks, and the special object of the author in bringing the communication before the English public was, to have the identification established between those lower rocks on which the coal fields rest, and those which support our great carboniferous series in the British Isles. The president then pointed out on the map the places where the different fossils were found, and showed that in many respects there was a striking coincidence between the stratifications and the remains discovered in them, with those of our own country. One of the most remarkable coincidences was a cluster of fossil trees, the representation of which resembled those in Dixon Fold so much, that it was supposed by the meeting, until the president informed them to the contrary, that the diagram referred to that group. In the course of a conversation which afterwards ensued, Mr. Binney noticed one difference: the fossil trees in Dixon Fold had only four roots each, while the American fossils had many. Mr. Bassett, a gentleman who was present when the trees were sketched, stated that their respective diameters varied from one foot to eighteen inches. After some observations from Professor Sedgwick, Professor Phillips, Dr. Buckland, and Sir H. De La Beche, the thanks of the meeting were unanimously given to Dr. Owen for his communication.

Fossil Head of an Ox.—The president next exhibited the head of an ox, and read a letter from Professor John Knox, stating that the head was found a few days ago in excavating a sea-lock at the east end of the Forth and Clyde Canal, on the Frith of Forth, at twenty feet below high-water mark. Mr. Thompson, the engineer of the Canal Company, happening to be present at the moment it was discovered, had it placed in a box and forwarded to the association.—Dr. Buckland said it was apparently the head of a cow, and was found under a large quantity of mud, at the mouth of an estuary,

where it was not at all surprising that mud should have been accumulated.

British Belemnites.—Professor John Phillips, as one of the committee for advancing the knowledge of belemnites, reported what had been done in that respect since the last meeting of the association. He had been requested to prepare a series of drawings and descriptions, of an exceedingly numerous group of fossil shells, known by the name of belemnites. M. Du Blanville had already described a great number of these fossils, traced the analogy between their structure and that of other fossils, particularly the nautilus, to which the analogy was striking. M. Voltz, of Strasburg and Paris, also published a valuable work on this subject, and Dr. Buckland, in the Bridgewater treatise, had added much to our knowledge of those animals. Fifty pounds had been placed in the hands of Professor Phillips to defray the expense of drawings and engravings; and these were nearly completed, and would be distributed among geologists. He had already collected forty apparently distinct species of the belemnites. The belemnite had four aspects, two of which were alike, and two dissimilar; and he would have to make such drawings as would enable a person to see all the aspects in which they could be presented, in order that the species might be distinguished. He hoped he should soon be able to give a correct account of the forms of the belemnites, and the manner in which they are distributed in the stratified rocks.

Fossil Fishes.—Mr. L. L. B. Ibbetson read a report of Professor Agassiz on the fossil fishes of the Devonian, or old red sandstone formation. The learned professor commenced his paper by acknowledging the assistance which he had received from various quarters, particularly the late Lady Gordon Cumming, who had presented him with several splendid drawings of fossil fishes executed by herself and her accomplished daughter. These beautiful works of art were exhibited to the section, and greatly admired: six of them were representions of *Pterycthys*, or winged fishes. The views presented in this report are similar to those found in a popular little work by Mr. Hugh Miller, of Cromarty, entitled "New Walks in an Old Field."

Mud of Rivers.—The president informed the section that the committee appointed to report on the mud of rivers, had forwarded a communication from Belfast, stating that they had as yet made little progress, as the results were very unsatisfactory. With regard to earthquakes, they had nothing to report. A great number of those stories appearing in newspapers were nothing more than exaggerated accounts of storms.—The section adjourned at half-past two o'clock.

SECTION D.—ZOOLOGY AND BOTANY.

The Dean of Manchester, President.—Dr. G. Lankester, F. L. S., drew the attention of the meeting to a gigantic pair of the horns of

Wapeti deer brought from the western districts of America, which were exhibited by Mr. L. T. Gaskill, of Liverpool. Being placed in front of the chairman, they attracted great attention from their immense size; the head being nearly three quarters of a yard in length, with tufts of hair on each side, almost resembling the front mane of a lion; whilst the towering antlers were about six feet in length, from the base of the scull to the tip of the horn.—Mr. Gaskill stated that these immense antlers were brought hither by Mr. Daniel Mossinore, an eminent naturalist of the United States, along with the living male and female of the same species (young ones), which are now in the possession of her Majesty, in Windsor Park. It appeared from Mr. Gaskill's statement, that his Royal Highness Prince Albert, hearing of the arrival of the deer at Liverpool, instructed his secretary, the Hon. A. Murray, to make inquiries concerning them; but in the mean time they were purchased by the Earl of Derby, president of the Zoological Society, who also wished to become possessed of this extraordinary growth of animal nature; which had, however, become the property of the present owner. The noble earl hearing of her Majesty's desire to purchase the deer, with a proper feeling of loyalty, immediately presented them. The horns excited an interesting discussion.—Mr. Peach read a paper on the nidus and growth of *Purpureus Capillus*, the tea cup of Ellis.—Mr. Couch, of Cornwall, read a paper on the migration of birds and flowering of plants in that county.—Mr. Blackwall described a larva parasitic on spiders.—Mr. Strickland read the report of a committee, appointed by a late meeting, on the growth and vitality of seeds.

SECTION E. MEDICAL SCIENCE.

There were only three papers announced for reading in this section, and none of them possess any interest for non-medical readers.

FRIDAY.

SECTION G.—MECHANICAL SCIENCE.

The Combustion of Coal and the Prevention of Smoke.—Mr. Fairbairn read the report of a committee appointed at the meeting held at Glasgow, in 1840 (in consequence of a paper read by Mr. C. W. Williams), to make experiments upon the combustion of coal and other fuels, with a view of obtaining the greatest calorific effect, and avoiding the generation of smoke. In this report the subject of inquiry was arranged under the following heads:—1. The present state of knowledge as regards the combustion of fuel, particularly as applied to the furnaces of steam engines.—2. The due relations between the dimensions of the furnace and the boiler.—3. The dimensions and form of the chimney.—4. The best method of working the furnace. In reference to the general subject of the inquiry, the report stated that very great ignorance existed on the subject of combustion, and

a most extravagant waste of fuel was committed in most of the districts in which steam power is extensively used. In Cornwall, where necessity had compelled the introduction of great economy in the management of steam engines, a system of slow and complete combustion was practised which unfortunately could not be generally adopted in this district, where most of the proprietors of steam engines had subjected themselves to serious evils by a want of capacity in their boilers, and a system of overworking in every way opposed to perfect combustion, and attended with consequences equally objectionable, whether as regarded the public nuisance of smoke, or the increased expenditure of fuel. In many establishments, engines were loaded to almost double their nominal power; requiring steam far beyond the just capability of their boilers, and to be obtained only by forcing their fires, and too frequently dispersing through the atmosphere a quantity of valuable fuel, which under other circumstances, might be usefully and beneficially employed. These were evils which could be abated only by the zealous concurrence of proprietors of steam engines themselves; but it was to be hoped, that a few striking examples of great economy and perfect combustion would have considerable effect in directing the attention of these parties to a due consideration of their own interest and of the public welfare. The report then went into a minute description of the relative proportions of furnaces, boilers, flues, and chimneys, in this and other districts, from which we select the following points. In Manchester, the usual proportion of the area of the fire bars to the heating surface of the boiler is about 1 to 11, or 100 square inches to 8 square feet of flue per horse power. In a well proportioned boiler, on the plan usually adopted in this district, a pound of moderately good coal will evaporate about 7·46lb of water, being nearly the maximum duty effected here. A number of experiments had been made upon engines in this town, the results of which were given in a table exhibited to the meeting, specifying the nominal power of each engine, the power at which it was worked, the proportion of heating surface to the grate bars, the height of the chimney, the consumption of fuel per horse power, and some other particulars, none of which, however, seemed to throw much light upon the great question of economical combustion. The greatest consumption of coal (13lb per horse power per hour) was in an engine of 45 horse nominal power, worked up to 76 horse, and having the grate bars in the proportion of 1 to 10·87 of the heating surface of the boilers. The smallest consumption (8·8lb per hour) was in an 80-horse engine worked to 110 horse, and having its grate bars in the proportion to the flues of 1 to 12·70. The second, in point of economy, however, (9·1lb per hour), had the smallest proportion of heating surface of the whole number experimented upon. The general average of the consumption of coal was 10·53lb per horse power per hour. That this was far beyond what it ought to be, was exhibited by the fact, that the average con-

sumption of the best Cornish engines was only 2½lb per hour—something less than one fourth of the amount in this neighbourhood. The report proceeded to detail the results of some experiments on the comparative consumption of coal in a furnace to which Mr. Williams's apparatus for the consumption of smoke had been applied, when that apparatus was at work, and when it was thrown out of use. Some of these experiments appeared to be vitiated by the want of a perfect closing of the air passages when the apparatus was not used. Further experiments were consequently tried with the air passages open at one time, and at another closed by a brick wall. The result was, that the average consumption with the apparatus at work, was 276lb per hour, and with the air passages effectually closed, 308½lb per hour; showing a difference of 32½lb per hour in favour of Mr. Williams's plan, or a saving of rather more than 10 per cent. The report stated, in conclusion, that there could not be the slightest doubt about the practicability of abating the nuisance of smoke so much complained of in this and other districts.

The reading of the report of the committee, and the explanations of the different plans of consuming fuel which followed, gave rise to a very long discussion, of which we only present a few of the leading points to our readers.

Mr. C. W. Williams said, that, in considering the questions of the most economical combustion of fuel, and the prevention of smoke, it would be well to discard all consideration of the boiler, and the generation of steam, which were apt to introduce complexity and uncertainty into the inquiry. The question first to be considered was, how the most perfect combustion could be attained; how, from a given quantity of fuel, the greatest quantity of water and carbonic acid could be produced, and the public nuisance of smoke could be best abated. The application of the heat obtained to a boiler with the view of evaporating the greatest quantity of water was a separate question, and should have an entirely separate consideration. With respect to perfect and imperfect combustion, he thought the whole subject was explained by the little instrument he held in his hand (a short piece of copper gas tubing, with an argand burner at one end). Supposing a stream of gas to pass through the tube, if he took off the burner, and set fire to the gas, there was imperfect combustion, and a large emission of smoke; if he put on the burner, so as to cause the gas to issue in a thin sheet, and thus receive an abundant supply of oxygen, there was a perfect combustion and an entire absence of smoke. He thought that patentees who came forward to ask the public to patronise their inventions should, like himself, explain the mode of their operation; and he would beg to ask Mr. Juckes what was the chemical operation of the plan which he had submitted.—Mr. Juckes said he was no chemist: he could only refer to the results of his patent.—Mr. Williams said he doubted whether mere experimental results could be at all satisfactory. He

believed there was not one of the plans for preventing smoke, which had been submitted to the public, that would not answer the purpose when it was carefully worked; and, as to the saving of fuel, the difference between a dry and a moist day, between large coal and small, between judicious and injudicious firing, would easily make a difference of ten or fifteen per cent. in the consumption.—Mr. West, of Leeds, said that the great point for consideration was the prevention of the public nuisance arising from smoke: he had recently paid great attention to this subject; he had seen nine plans for effecting it in actual and successful operation; he was perfectly satisfied, that, by the adoption of any one of them, nine-tenths, if not ninety-nine-hundreths, of the smoke now made might be prevented, to the great advantage of the owners of steam-engines, as well as of the public.—Mr. Henry Houldsworth said that, for six months past he had practical experience of the working of Mr. Williams's patent, which he had applied to six different furnaces; and he could now say confidently, from the results of that experience, that without any particular trouble or care of management, it would prevent, at the very least, three-fourths of the smoke which was now made. He did not doubt that other inventions might be equally effectual when they were carefully managed; but he preferred Mr. Williams's, because of its extreme simplicity, depending as it did solely on the admission of air in a proper manner, without any of those mechanical contrivances, worked by some moving power, which many other plans contained. There was one fact connected with Mr. Williams's patent, which he considered of some importance, which he would communicate to the section. He had that morning fitted up a contrivance for ascertaining the comparative temperature of flues under different circumstances, which had not previously been very satisfactorily ascertained. Mr. Williams had used a thermometer, inserted in a bar of iron, which was placed in a flue; but he (Mr. Houldsworth) was not satisfied with that plan, and had passed a copper wire through the flue from one end to the other. This was kept in a state of tension by a weight, and by its expansion or contraction, acted upon an index, which would give a very correct measure of the relative temperature. He had tried some experiments with it that morning, and had obtained very striking and important results. It had generally been supposed, that, when there was a perfectly red fire in the furnace, and when no smoke was generated, the admission of cold air at the bridge would do harm instead of good, by reducing the temperature in the flues. He had, however, tried the experiment that morning. After having the air passages closed for some time, he had opened them when the coals on the fire were perfectly charred, and found an immediate and decided increase of temperature in the flue. The increase of temperature was certainly most striking if the air passages were opened shortly after a large quantity of fresh fuel had been put on; but at all times he found there was an increase when the air was admitted,

and a decrease when it was excluded. If any members of the association would do him the favour to call at his works, he should have great pleasure in showing the apparatus and its working.—Mr. Williams said, he was exceedingly glad to hear of Mr. Houldsworth's invention: a good pyrometer was very much wanted.—In answer to a question from Mr. E. Corbett, Mr. Houldsworth stated that, in one experiment, about three cwt. of coals were thrown upon the fire at once, so as to produce a thick black smoke; when the air passages were closed, that smoke was immediately dissipated by opening them, and the temperature rose rapidly. On closing them again, the smoke returned, and the temperature as rapidly declined.—Mr. Taylor (the treasurer of the association) said, the subject before the section was undoubtedly of great importance to the manufacturers of this district; for they had already heard from Mr. Fairbairn, that, whilst the average consumption of ten engines in Manchester was 10½lb of coal per horse power, per hour, the average consumption in the Cornish engines was only 2½lb, so that they did as much work with one pound of coal as a Manchester manufacturer did with four pounds; and at the same time they made very little smoke. In the parish of Warnup, with which he was connected, there were twenty-five engines, all of very large power, from the chimneys of which very little smoke would be seen. He did not know that they adopted any contrivance for the prevention of smoke, beyond that of admitting air at the bridge: but they gave plenty of furnace room and plenty of boiler room, and made a rule of keeping their fires bright, and of cokeing all their coal in the front of the furnace. The great source of their superiority was, however, in his opinion, to be found in the fact, that as the engines were all employed in pumping water, it was exceedingly easy to ascertain the amount of work which each engine performed with a given quantity of coal. This was invariably recorded and published, and it produced great emulation amongst the engine-makers, the proprietors, and the workmen, each of whom was ashamed of being outdone by his neighbour; and from their joint care and exertions, and most especially from those of the workmen employed, resulted that extraordinary economy of which the section had just heard.—Mr. Richard Roberts said he quite agreed with Mr. Taylor, that with due care on the part of the fireman, a sufficient area of grate, and sufficient boiler room, smoke might be prevented without resorting to any particular contrivances.—After some further discussion, in which Mr. Edmund Ashworth, Mr. Webbe Hall, Mr. Joshua Milne, and other gentlemen took part, Mr. Jeremiah Garnett said, he could not permit the discussion to close without directing attention to one fact, namely, that although they had a variety of opinions about the best mode of preventing smoke, no one among the many scientific men and manufacturers present intimated the slightest doubt that smoke might be prevented, and the nuisance which so greviously affected Manchester and its neigh-

bourhood, if not entirely removed, might be exceedingly diminished. If, therefore, parties working steam-engines would neither consult their own interests nor consider the public welfare, he trusted the legislature would interfere and compel them. There was one point adverted to by Mr. Taylor and Mr. Roberts, on which he would say a few words, as it might do harm out of doors. They had mentioned an abundance of grate and boiler room, as a means of preventing the nuisance of smoke, and no doubt it was so. But there was a danger that parties who were deficient in those respects might plead that deficiency as an excuse for continuing the nuisance. It was therefore necessary to remind the section, that, though desirable, an abundance of grate and boiler room was not necessary to the prevention of smoke. He knew from Mr. Houldsworth, that the very successful experiments made by him were made upon engines which were very limited in those respects. What had been done by Mr. Houldsworth could be done by any other parties; and, therefore, those who continued to poison their neighbours with smoke were without excuse.

The section was then adjourned to the following day.

(To be continued).

Voltaic Pile of Plate Iron. *By* M. Wöhler.*

Professor Poggendorff has shown that the expensive platinum plates in Grove's Battery, may be substituted by plates of iron, without much diminution of power: and the subsequent researches of Wöhler and Weber have shown that plates of iron may also substitute the plates of amalgamated zinc. These philosophers thought that iron in the concentrated nitric acid would operate with iron in the diluted sulphuric acid as the platinum to the amalgamated zinc in the original battery of Grove. It is said that this view of the action has been verified by means of a pile constructed with iron and these two liquids; which the projectors have found to possess *great power;* a circumstance of great interest, whether viewed with reference to the theory of the voltaic pile in general, or to the voltaic action of iron in particular. Every one will now be able to make a voltaic pile, of great power and constant action, at a trifling expense: it being only necessary to procure a few curved plates of iron and porous earthen cylinders. The concentrated nitric acid is the only article of much expense.

The plates should be cut from strong unoxidized iron, bent into cylindrical forms, and those of each pair united by a metallic strip. The smaller cylinder is placed in an earthen pot containing the nitric acid; and the larger iron cylinder is placed in a glass goblet containing diluted sulphuric acid, in the centre of which is also placed

* Extract from *Liebig's Journal.*

the former part of the apparatus. The larger iron cylinder exposes only about three square inches of surface; yet two pairs of this description, is sufficient to heat to whiteness two inches of *fine* platinum wire, and to decompose water *very rapidly!!!*

There is a trifling liberation of hydrogen gas at the plate immersed in the sulphuric acid; but it may be avoided by having the iron tinned. The coat of tin on the iron answers to mercury on the surface of zinc, and produces a similar effect: it seems even preferable!!! Probably the apparatus would be better if made of cast iron. The porous cylinders which are employed in this battery are those made at Berlin, and are of excellent quality, and apparently of calcined porcelain. When these cannot be had, they may be substituted by Hessian crucibles.

The nitric acid employed in this battery should be sufficiently concentrated to prevent its attacking the iron. A mixture of one part of fuming nitric acid, and one and a half of common nitric acid, will answer very well.

Note.—This is not the first time of our hearing of voltaic batteries of "*constant action,*" with iron as the only metal; and, we believe, all of them have been planned from the idea of nitric acid of a certain strength not acting on iron: without ever considering that the acid by voltaic action soon becomes altered. But this is the first time of our hearing of the substitution of *tinned iron* for amalgamated zinc!!!—EDIT.

On Metallic Acids. *By* M. E. FREMY.

(Abstract from a Memoir read before the Academy of Sciences).*

FROM a general examination of the metallic acids, I have been led to the discovery of some new compounds of metals and oxygen, and consequently of new salts, some of which are remarkable for their beautiful crystalline forms. The metallic acids may be classified under two distinct heads: the first of which comprehends all those which are formed by the immediate combination of metals with oxygen, and are soluble in the alkalies at common temperatures. The second class consists of those acids which are formed by the combined action of an alkali, an oxydizing body, and a metallic oxide, when simultaneously in contact with one another.

These two classes of metallic acids, display properties essentially different from each other. Those of the first class are generally stable; and in combination with certain bases are productive of well-defined crystallized salts. The second class, on the contrary, suffer partial decomposition by a loss of oxygen, when exposed to the feeblest influences.

To give an example of the first class of these acids, I have selected the last combination of tin with oxygen, which has obtained

* *Comptes Rendus.*

the appellation of *stannic acid.* The second class of metallic acids I shall illustrate by a new compound of iron with oxygen, which I have named *ferric acid.* In making these selections of acids, into which the known *important* metals enter as essential elements, I have been desirous of showing that similar compounds are obtainable with metals less frequent to observation.

I have commenced with the examination of *ferric acid,* and have given a minute description in my memoir of the different processes by which I have obtained the *ferrates.* The compounds of ferric acid with bases may be formed either by the dry or by the humid way. The dry method is simply that of calcining the peroxide of potassium with a sesquioxide of iron, in any vessel not operated on by the produced ferrate. The ferrate of potassa may be easily obtained by the dry process, by throwing ten parts of dry powdered nitre on five parts of red hot iron filings. The red mass thus obtained contains a large share of ferrate of potassa. I have obtained the ferrate of potassa, by the humid process by availing myself of the results of M. Bertheir's experiments, which show the action of chlorine on the metallic oxides. I transmit chlorine through a concentrated solution of potassa, which holds the hydrate of peroxide of iron in suspension. By these means the ferrate of potassa is produced.

In this part of my memoir I enter into some details on the action of chlorine, and strong solutions of potassa; and show, in this case, contrary to the general opinion, that there is no formation of chlorate of potassa and chloride of potassium; but that there is formed a peculiar compound, which I have named *potasse chlorée,* which at a slight elevation of temperature becomes resolved into chloride of potassium, oxygen, and potassa. It is this compound which, by reacting on the hydrate of the peroxide of iron, gives rise to the formation of ferrate of potassa. In my memoir I dwell upon the influential action of chlorated potassa, in the production of new compounds, constituted of metallic acids and suitable bases; and, in illustration, I prove that under the influence of chlorated potassa, the oxide of copper becomes transformed into a new metallic acid, which I have called *cupric acid,* and that this acid forms a new compound with the potassa. In other respects it has not been my object to pay particular attention to the action of chlorine on the alkalies; this study belonging more properly to those chemists who have recently brought to light so much interesting matter in this branch of science.

I next proceed to an examination of the properties of the ferrates, and prove that heat, organic substances, or bodies in a state of minute division, are capable of decomposing the ferrates. I also compare these reactions with those which take place in peroxide of hydrogen under similar circumstances. The composition of ferric acid is represented by the formula $Fe\,O^3$. This acid may therefore be classed with the chromic, manganic, sulphuric, &c. By analysis I prove that the ferrates, by whatever way produced, have precisely

the same composition, but that those obtained by the dry process are generally mixed with nitrite of potassa, which absorbs oxygen at the moment the ferrate commences decomposition, and thus becomes a nitrate of potash.

Lastly, I describe all the experiments that I have made whilst in pursuit of an acid with a larger proportion of oxygen than is contained in the ferric acid, or of an oxide corresponding with the peroxide of manganese. I allude to the action of the binoxide of barium on the sesquioxide of iron, and advance reasons which lead to the opinion that, in that case, there is formed a compound of iron and oxygen, intervening sesquioxide of iron and ferric acid. Such are the various topics of which I have treated in the first part of my memoir. The second part is devoted to an examination of stannic acid. I precede this examination by reviewing all that has been made public concerning that acid, and especially the remarkable researches of Berzelius, and the interesting observations of Gay Lussac thereon. I also allude to a note in *Liebig's Journal*, by Mr. Graham, in which he explains the modification of stannic acid, previously made known by Berzelius. The first experiments that I made on stannic acid were intended to ascertain what part it plays in its combinations: for chemists have hitherto held different opinions respecting its influence in that capacity. Ought stannic to be considered as an acid, or as a base? May it not alternately operate as an acid and as a base? I have directed my attention to these inquiries.

The result of all the experiments to which I have submitted stannic acid are decidedly unfavourable to its being considered as a base. If, for instance, it be formed by decomposing chloride of tin with an insoluble carbonate, a well developed acid, capable of reddening turnsole, is precipitated. But if the chloride of tin be treated with carbonate of potassa, no stannic acid is produced, the result being a stannate of potassa, which is insoluble. In examining the compounds consisting of stannic acid and the ordinary acids, I find that they ought not to be regarded as salts of peroxide of tin, but rather as combinations of stannic acid with those acids. It is known that chemistry affords many examples of combinations of acids by which double acids are formed. M. Chevreul has proved that stannic acid placed in contact with the colouring matter of logwood, operates in the capacity of an acid; whilst the metallic oxides, properly speaking, even the peroxide of tin itself, operate in the capacity of bases. Hence the compound in which oxygen unites with tin in the highest degree should always be regarded as an acid.

After the examination of this first topic in the history of stannic acid, I proceed to the consideration of its properties. The experiments which I first describe are intended to elucidate the cause of the modifications presented by stannic acid. The inquiry also applies to other metallic acids, and gave rise to the investigations of Berzelius; and, from its generality, involves a problem whose resolu-

tion is of high importance. From the results of my experiments I learn that the two modifications of stannic acid, are, in fact, distinct acids, to which I have given different names. To that produced by nitric acid, I have continued the name *stannic acid,* and that obtained from the chloride of tin I name *metastannic acid.*

In determining the proportions of water contained in these two insulated acids, I have ascertained that the metastannic acid is more hydrated than the stannic acid; and as the only difference in their constitutions is in the proportions of water which they contain, it is obvious that by gentle dessication, the metastannic acid may be transformed into stannic acid. Applying to these acids the ingenious idea of Graham, with reference to phosphoric acid, I was led to infer that the stannates and the metastannates would be found to differ from each other only in the proportions of base, which has proved to be the fact; for by representing the neutral stannates by the general formula, $Sn^3\ O^6\ MO$, the composition of the metastannates would be represented by $Sn^3\ O^6\ {}^3MO$. Hence, accordingly with this hypothesis, which I discuss at some length in my memoir, the stannic acid should be regarded as a monobasic acid, and the metastannic as a tribasic acid. The relation which exists between the stannates and the metastannates, explains a curious fact which I have observed, viz.: the stannates when heated with an excess of alkali become transformed into metastannates. The stannates may be obtained by dissolving the alkalies in cold stannic acid which has been prepared by the action of nitric acid on tin. The metastannates may be produced by two distinct processes. By dissolving, in alkaline solutions, the metastannic acid which is obtained from chloride of tin, by the employment of an insoluble carbonate; and, secondly, by calcining in a silver crucible, the stannic acid with an excess of base. The metastannates of potassa and of soda crystallize with facility. These compounds are not surpassed, in any respect, even by the most perfectly defined salts, and probably are the best specimens of crystalline structure exhibited by the combinations of tin.

Whilst studying stannic acid, I have discovered a compound of tin and oxygen intervening the protoxide of tin and stannic acid, which must not be identified with the sesquioxide of tin recently discovered by M. Fuchs. This compound is obtained by treating stannic acid, at common temperatures, with protochloride of tin. The acid immediately assumes a fine orange yellow colour, and pure hydrochloric acid remains in solution. This compound, whose properties I have described in my memoir, should be considered as a stannate of protoxide of tin, corresponding to the molybdate of oxide of molybdenum, to the tungstate of oxide of tungsten, to the chromate of oxide of chrome, &c.

Finally: from an examination of the results obtained by the decomposition of the stannates by heat, and extending the process to other metallic salts, I have arrived at the following general conclusion, viz.: certain combinations of metals with oxygen are

not acid, unless hydrated; but when thus acidified, the water enters as a constituent element; and, contrary to that which occurs with other acids when combining with bases, it is not displaced by them, but continues as an essential part of the resulting salt. If by an elevation of temperature, the metallic acid loose its water whilst in combination, it will no longer remain united with bases, but is precipitated in the anhydrous state.

On the Formation of Ferric Acid, by Voltaic Electricity. By Professor J. C. Poggendorff.*

An examination of the chemical action of the voltaic pile has led me to discover a novel fact, equally interesting to physical and chemical science. When wrought iron of the best quality is placed in a solution of potassa, and united to platinum immersed in nitric acid, it liberates oxygen gas from the solution, without suffering oxidation itself. Oxygen is also liberated in the gaseous form, when plumbago, platinum, palladium, gold, nickel, cobalt, or tin, is substituted for the iron. Silver, copper, antimony, bismuth, lead, cadmium, and, what is still more remarkable, even zinc, liberates gaseous oxygen: though, not without suffering a partial oxidation, and consequent alteration on the surface of the metals. Silver and lead, which soon become covered with a film of black oxide, are striking examples of this action; and it is not until after this covering is formed, that these metals liberate oxygen gas.

With *cast iron*, which, by accident, I was led to use in my experiments, the case is very different. This metal is immediately enveloped in a wine-red coloured cloud, which gradually expands till the whole of the liquid assumes a red tint; growing darker as the voltaic action proceeds, and eventually becomes of a deep brown colour, approaching perfect opacity, excepting at the edges, where the light still shows the beautiful *medoc*† colour. The concentration of the alkaline solution appears to be of little consequence in the production of this phenomenon. I have produced it in all its splendour and beauty in a solution of one part of hydrate of potassa in four parts of distilled water.

By a close examination of the alkaline solution, a feeble cracking noise amongst the air bubbles will be heard; and, at the same time, a change of colour in the liquid will be observed to be taking place. The colour assumes a reddish brown, which gradually becomes deeper, but, in about half an hour, the colouring matter having all fallen to the bottom of the vessel, the liquid becomes quite colourless. A continuance of the electric current does not prevent, nor even retard this change; for it commences and goes on whilst

* *Annalen der Physik und Chemie.*

† The French name for a beautiful red coloured flint pebble, found in Madoc.—Edit.

the voltaic action of the iron and platinum is in full vigour. If the solution be heated to the boiling point, the precipitation of the coloured matter takes place immediately. Zinc immersed in the red liquor, also accomplishes precipitation, and, in a short time, the liquor becomes colourless.

At first, I was inclined to attribute this phenomenon to the presence of manganese in the iron; but from a close examination of the circumstances under which it was exhibited, and more especially from a chemical analysis of the brown precipitate, in which nothing but oxide of iron could be detected, I am led to infer that the colouring of the liquid was due to the formation of ferric acid; or rather to that of ferrate of potassa. The production of ferrate of potassa, under these circumstances, may be easily explained. It may be attributed to a predisposition of the oxygen to combine with the iron, by which, and the influence of an electric current, ferric acid would result: which, at the moment of its formation, would combine with a portion of the potassa of the solution; or the electric current may, in the first place, enhance the affinity between oxygen and potassium, and thus give rise to the formation of peroxide of potassium, and afterwards to the ferrate of potassa. This latter explanation appears to be correct, from the fact that, if ammonia be used, ferric acid is not obtained. The phenomenon is interesting on two accounts: first, because we are now, by the aid of voltaic electricity, enabled to form an acid with facility, which M. Fremy, its first discoverer, and other chemists, have hitherto obtained by purely chemical means, only with greater difficulty: and secondly, because it is obtainable only by the employment of cast iron, and not by wrought iron.

This latter circumstance is certainly very enigmatical. I have entered into extensive experimental inquiries for the purpose of ascertaining the cause; but, hitherto, without success. I have never been able to produce ferric acid by employing either wrought iron or steel; neither have I been successful with every kind of cast iron. I have employed four kinds, but with two of them only have I succeeded. This difference in the results, by the employment of different kinds of iron, seemed for some time, to be attributable to a difference in the force of the electric currents; but from a comparison of the electric forces given by a pile in which *wrought iron* was one of the metals, with those by a pile into which *cast iron* entered as an element, I found that only a very trifling superiority was due to the latter: that by which ferric acid was producible.

By lengthening the conducting wire of the cast iron battery, I reduced the power of the current to below that of the wrought iron battery; but the ferric acid was no less produced by the former though not by the latter.

In a theoretical point of view, the great similarity in the force of the currents produced by the two kinds of iron battery, appears to me, to be an interesting circumstance: for whether the liberated body (in this case oxygen), be insulated and in a free state, or it be

combined with the metals, the force of the current, and the electromotive power, seem not to be affected. This fact is very unfavourable to the opinions of Grove and others, who think that water is more easily decomposed by the electric current when oxygen combines with the positive metal of the pile. At least, it is favourable to nothing further than the generally received opinion that, in the chemical combination of two bodies, there is a great quantity of electricity brought into action. It is to be understood, however, the current must have a certain degree of intensity for the production of ferric acid. I immersed two plates of cast iron in a solution of potassa, and afterwards united them to the metals of one of Grove's simple piles, but in consequence of the small dimensions of the pile the current was too feeble to produce any more than a very small trace of ferric acid. On the positive plate, however, there was attached a small portion of oxide, an incident I have never had to occur on wrought iron; and proves that, under these circumstances, cast iron has a greater tendency to oxidize than wrought iron.

In general, cast iron, and especially that kind by which ferric acid is most abundantly produced, is more soluble in acids than wrought iron: probably in consequence of the particles of carbon which are intermixed with it producing voltaic action, and thus facilitating its solution; as in the case with commercial zinc, the impurities of which cause it to dissolve more readily than pure distilled zinc; and it is possible that the production of ferric acid, by cast iron, may be traced to the same cause.

An increase of intensity in the current, to a certain extent, at least, does not interfere with the production of ferric acid. A current from two of Grove's batteries, transmitted through a solution of potassa, between cast iron terminals, produces a rapid formation of ferric acid.* This mode of obtaining the acid, I consider preferable to that which I first resorted to; as it is not necessary to put the solution of potassa in contact with an argillaceous vessel, which, consequently, prevents it from becoming impure, and the vessel from destruction, which invariably happens when employed. I must here state, however, that this process is attended with some little inconvenience, arising from the loss of a portion of the ferric acid, by decomposition at the negative terminal metal. This loss, however, is amply compensated by the superabundance of acid which is formed at the positive cast iron terminal. I have called the loss sustained at the negative terminal a trifling one, which, to some persons may probable appear an inappropriate description, by supposing that as much ferric acid would be decomposed at the negative terminal as is formed at the positive one; but this is not the case.

* A curious modification of this process is made by employing one cast iron plate, and one of wrought iron. When the latter is the positive terminal in the alkaline solution, oxygen gas will be liberated; but when the former is made the positive terminal, ferric acid will be found.

There is always a greater proportion of hydrogen than of iron liberated at the negative pole; hence, a greater proportion of water than of ferric acid is decomposed.

On the other hand, notwithstanding the formation of ferric acid, however abundant, oxygen gas is invariably liberated at the positive terminal plate; and this occurs under all circumstances, whether we employ a battery or only a single voltaic pair; but, in all cases the battery liberates the greater quantity of oxygen gas: it is therefore possible that, by a sufficient increase of intensity of the current, oxygen gas only, and no ferric acid would appear, even with cast iron terminals. This copious liberation of oxygen gas is very unfavourable for ascertaining the composition of ferric acid obtained by the employment of voltaic electricity.

We may rest assured, however, that the quantity of oxygen determined at the positive terminal metal, is the equivalent of the hydrogen liberated at the negative terminal; and if the former were wholly to combine with the iron (whether permanently or not, would be of little consequence), the quantity of liberated hydrogen would be a representative of the quantity of oxygen in the ferric acid; and by deducting the oxygen from the weight of the oxide formed, the quantity of iron in the compound would be determined. But the liberation of oxygen gas, and the uncertainty of its being *wholly* engaged in the formation of ferric acid, precludes the possibility of applying this process of analysis.

The ferric acid being susceptible of decomposition in so eminent a degree, by the feeblest influences, I have not attempted the insulation of either itself, or its salt. If there be a possibility of their insulation, it can be accomplished at low temperatures only.

In conclusion, I think there is a probability of ferric acid being found, ready formed in nature, and constituting the colouring matter of the amethyst, in which, besides silica and a trace of manganese, iron has also recently been discovered.

On the Preparation of Calomel. *By* M. Sonberian.*

In France and in England, medical practitioners, with very few exceptions, administer that calomel which is prepared by steam: for they find it more active and more certain in its effects than that prepared by other means. The method of preparing this calomel is that shown by Joseph Jewel, modified by M. Ossian Henry, and consists in conducting a mixture of steam and the vapour of calomel into a capacious receiver: a process carried on in France to a great extent. I have practised this process for many years at the *Pharmacie Centrale*, but by no means approve of it: for it is difficult to conduct, requires much dexterity of manipulation, and is frequently attended with accidents, by which a considerable portion of the

* Comptes Rendus.

product is lost. Moreover, the calomel prepared in France is neither so white nor in such a fine pulverulent state as that which we receive from England.

I have now to communicate to the Academy, a novel process for preparing calomel, which is very superior to any hitherto made known. It is of a peculiar character, and as it is applied to the production of an article in which we have never yet been enabled to compete with the English manufacturer, I hope the Academy will not think the description of it an intrusion on their attention.

Instead of the vapour of water being interposed between the particles of the vapour of calomel, which prevents them from uniting, as in the steam process, I substitute a current of air, which, by passing over the heated calomel, enters its vapour as it forms, and causes it to condense in an impalpable powder. I heat the calomel in an earthen tube, which passes through a furnace, and through the heated tube I propel a current of air, by means of a small blowing apparatus, which mixes with the vapour, and is driven with it into the receiver. Should the tube be perfectly straight, a portion of the calomel would be carried to the distance of twenty yards: but to prevent this inconvenience I bend the further end of the tube, so that it may terminate in a vessel of water; the calomel thus received by the water, deposits in a fine powder, and the end in view is accomplished. To perfect the process, it now remains only to ascertain the most suitable form, and the best material for the vessels in which the calomel is heated. Not having yet met with these essential qualities in any ready made vessel, I am not prepared at present to carry the description further; and must content myself with the complete success in my first attempts with this process; which I have no doubt will be found applicable in the minute division of the volatile bodies.

PROCEEDINGS OF THE ROYAL SOCIETY.

November 18*th*, 1841.

THE following papers were read:—1. "Variations de la déclinaison et intensité magnétique horizontale observées à Milan le 28 et 29 Mai, le 23 et 24 Juin, le 21 et 22 Juillet, le 27 et 28 Août, et le 22 et 23 Septembre, 1841." Par Sig. Carlini, For. Memb. R.S.

2. "Variations de la déclinaison magnetique et de l'intensité magnétique horizontale observées à Bruxelles le 23 et 24 Juin, et le 21 et 22 Juillet, 1841." Par M. A. Quetelet, For. Memb. R.S.

3. "Meteorological Register kept on board the Earl of Hardwicke, during a voyage from London to Calcutta and back to London, by Captain Alexander Henning." Communicated by Sir John F. W. Herschel, Bart., F. R. S., &c.

4. "Meteorological Register kept at Port Arthur, Van Diemen's Land, by Deputy-Assistant-Commissary-General Lempriere, from

Feb. 1, 1840, to Feb. 1, 1841." Communicated by Capt. Beaufort, R.N., F.R.S., Hydrographer to the Admiralty.

5. "Term Observations of the Variation, Magnetic Declination, Horizontal Intensity, and Inclination, at Prague, for June, July, August and September, 1841." By Professor Kreil. Communicated by S. Hunter Christie, Esq., Sec. R.S.

November 25.—The following papers were read, viz.—

1. "Explanation of the construction, positions, comparisons, and times of observation, of the Meteorological Instruments at the Royal Observatory, Greenwich, with which the Observations have been made that are contained in the sheets of Meteorological Observations, forms 1 and 2, for each month from 1840, November, to 1841, July, both inclusive, sent to the Royal Society in 1841, October 26." By George Biddell Airy, Esq., M.A., F.R.S., Astronomer Royal.

2 "On the Laws of the Rise and Fall of the Tides in the River Thames." By George Biddell Airy, Esq., M.A., F.R.S., Astronomer Royal.

The conclusions arrived at by the author, and stated in this paper, were derived from an extensive series of observations of the tides, made, on this suggestion, at the royal Victualling Yard, at Deptford, under the superintendence of Captain Shireff, R.N. The object of the first series of observations was simply to ascertain the times of high and low water, for the purpose of ascertaining the duration of the rise and fall of the tide: the height of the water was observed at every quarter of an hour, night and day, during half a lunation. The curves representing the law of rise and fall of the water were found to be different for high tides and for low tides; and both are sensibly different from the line of sines. The author then investigates mathematically the motion of a very long wave, such as a tide-wave, in a rectangular canal, whose section is everywhere the same, on the supposition that the extent of vertical oscillation bears a sensible proportion to the mean depth of the water; and deduces an expression for the vertical elevation of a particle at the surface. This expression supposes the canal unlimited at the end farthest from the sea. If the canal be stopped by a barrier, the expression changes its form. The formulæ obtained by the author enable him to explain a circumstance, hitherto perplexing, namely, that the age of the tide is different as inferred from the height of the high water, or from the time of high water; being always greater in the former mode of estimation.

3. "Register of Tides, observed at Coringa, from January 1st to June 30th, 1841."

4. "Meteorological Journal, from the 20th April, 1840, to the 29th April, 1841. Kept at the Falkland Islands on board H.M. Ketch "Arrow."

5. "Daily Thermometrical Observations at Cape Palmas, for May, 1841."

These last three papers were communicated to the Society by

the direction of the Lords Commissioners of the Admiralty.

November 30, 1841.—Anniversary Meeting. The following are extracts from the Report of the Council.

Awards of Medals.

The Council has awarded the Copley Medal for the present year to Dr. G. S. Ohm, of Nuremburgh, for his researches into the laws of Electric Currents, contained in various memoirs published in *Schweigger's Journal*,* *Poggennorff's Annalen*, and also in a separate work, entitled *De Galvanische Kette Mathematisch Bearbitet*,† published at Berlin in the year 1827. In these works, Dr. Ohm has established, for the first time, the laws of the electric circuit; a subject of vast importance, and hitherto involved in the greatest uncertainty. He has shown that the usual vague distinctions of intensity and quantity have no foundation, and that all the explanations derived from these considerations are utterly erroneous. He has demonstrated, both theoretically and experimentally, that the action of a circuit is equal to the sum of the electro-motive forces divided by the sum of resistances; and that whatever be the nature of the current, whether voltaic or thermo-electric, if this quotient be equal, the effect is the same. He has also shown the means of determining with accuracy the values of the separate resistances and electro-motive forces in the circuit. The light which these investigations has thrown on the theory of current electricity is very considerable; and although the labours of Ohm were, for more than ten years, neglected (Fechner being the only author who, within that time, admitted and confirmed his views), within the last five years, Guas, Lenz, Jacobi, Poggendorff, Henry, and many other eminent philosophers, have acknowledged the great value of his researches, and their obligations to him in conducting their own investigations. Had the works of Ohm been earlier known, and their value recognised, the industry of experimentalists would have been better rewarded. In this country those who have had most experience in researches in which voltaic agency is concerned, have borne the strongest testimony to the assistance they have derived from this source, and to the invariable accuracy with which the observed phenomena have corresponded with the theory of Ohm.

* 1. On the electric conductibility of the metals. (*Schweigger's Journal*, second series, vol. xiv.)

2. Experiments to discover the power of electro-magnetic multipliers. (Ibid. vol. xxv.)

3. Researches to ascertain the nature of unipolar conductors. (Ibid. vol. xxix.)

4. On hydro-electric currents. (Ibid. third series, vol. iii.)

5. Statement of facts destroying the relations which have been confusedly established between several galvanic properties, and particularly hydro-electric conductors. (Ibid. vol. v.)

6. Theory of galvanic currents. (Ibid. vol. vii.)

† A translation of this work will be found in *Taylor's Scientific Memoirs*, vol. ii.—Edit.

This accordance, it may be observed, is altogether independent of the particular hypothesis which may be adopted as to the origin of electro-motive force; and obtains equally whether that force is regarded as being derived from the contact of dissimilar metals, or as referable to chemical agency.

The Council have awarded one of the royal medals for this year, which had been proposed for the subject of chemistry, to Robert Kane, M.D., M.R.I.A., Professor to the Royal Dublin Society, for his paper "On the Chemical History of Archil and Litmus," published in the *Philosophical Transactions* for 1840.

It has been found that various lichens, which communicate no colour to pure water, strike a fine blue with solution of ammonia. The valuable colouring matters archil, litmus, and cudbear, are commercial preparations of these lichens. Some progress had already been made in the investigation of their colouring principles by the labours of Robiquet, Heeren, and Dumas, of which the most important step was the discovery of *Orcine*, and also of *Orceine*, into which the former is converted by ammonia; but the observations were isolated, and the whole subject was in the greatest obscurity. The present memoir by Dr. Kane records the first attempt to sketch a general history of the class: and, considering the great and peculiar difficulties attending inquiries into organic colouring matters, the attempt may be esteemed eminently successful. It proved an investigation of considerable intricacy and great extent, involving several hundred organic analyses; and it had been conducted in a manner highly creditable to the author's skill as an analyst. The paper contains an account of the discovery of a large number of new compounds, not less than twelve, derived from archil and litmus, together with the more exact discrimination of several others, already known, but imperfectly described. The distinction made of two *Orceines*, which have hitherto been confounded as one, is a striking result contained in that paper: while the observations on the action of chlorine and of nascent hydrogen upon several of the bodies described, open new branches of inquiry.

The objects which the author had in view in these inquiries were the three following: namely, first, to ascertain the primitive form of the colour-making substance in a given species of lichen, and trace the stages through which it passes before the coloured subtance is developed; secondly, to determine the nature of the various colouring substances which exist in the archil of commerce; and thirdly, to examine the colouring materials of ordinary litmus. He finds in the lichen *Roccella tinctoria* the following bodies, either pre-existing in the plant, or formed during the processes employed for its analysis: 1. Erythryline; 2. Erythrine (the pseudo-erythrine of Heeren); 3. Erythrine bitter; 4. Telerythrine; and 5. Rocelline (the Rocellic acid of Heeren). The properties and constitution of these substances are then described, and the chemical formulæ given which are deducible from their respective analyses. The author

finds the archil of commerce to consist essentially of three ingredients, namely, orceine, erythroleic acid, and azoerythrine; of each of the two former there exist two modifications, and there is in addition a yellow matter. After comparing his results with those obtained by Heeren, by an examination of the products envolved by his erythrine in contact with air and with ammonia, and stating reasons for some changes in nomenclature, the author gives the chemical formulæ resulting fron his own analyses of these different substances.

His inquiries into the constitution of ordinary litmus, which form the last division of his subject, lead him to the conclusion that that substance contains the principles designated by him as erythrolein, erythrolitmine, azolitmine, and spaniolitmine; and that the colouring constituents of litmus are, in their natural condition, red; the blue substances being produced by combination with a base, the bases in that of commerce being lime, potass, and ammonia; and there is mixed up in the mass a considerable quantity of chalk and sand. The details of the analyses of these several substances, and the resulting chemical formulæ representing their constitution, are then given.

The concluding section of the paper is occupied by an inquiry into the decoloration of the bodies which exist in archil and litmus. The latter of these, the author concludes, is reddened by acids, in consequence of their removing the loosely combined ammonia by which the blue colour is produced; and the so-called hydrogen acids liberate the colouring matter by their combining with the alkali to form bodies (either chlorides or iodides), with which the colouring matter has no tendency to unite. Hence it appears that the reddening of litmus is no proof that chloride of hydrogen is an acid, and that the double decomposition which occurs is the same in principle, whether hydrogen or a fixed metal come into play. After detailing the blanching effects of other deoxydizing agents on the colouring matter of litmus, and the action of chlorine on orceine and azolitmine, the author remarks, that in these actions chlorine is subjected to conditions different from those which determine the nature of the results with the generality of organic bodies, and that the displacement of hydrogen, so marked in other cases, does not exist in the class of substances under consideration; but that, in reality, the products of the bleaching energy of chlorine resemble in constitution the compounds of chlorine which possess bleaching powers.

This paper may be viewed as a very important contribution to organic chemistry, and as highly deserving of the royal medal; an award which will, doubtless, be hailed by chemists as a just encouragement to perseverance in skilful analytical research.

There being no paper on mathematics coming within the stipulations regulating the awards of the royal medals, which has been deemed worthy of that for mathematics in the present year, the council have, in virtue of the power given to them, under these

circumstances, by the regulations prescribed by her Majesty, awarded the other royal medal to Eaton Hodgkinson, Esq., for his paper which was published in the *Philosophical Transactions* for 1840, and is entitled "Experimental Researches into the strength of Pillars of Cast Iron, and other materials."

This paper has been esteemed by the council to be particularly valuable in a practical as well as theoretical point of view, and therefore to deserve, in an eminent degree, the honour of a royal medal. It contains the results of an immense series of experiments, conducted with great patience and admirable skill, and at a very considerable cost. Mr. Hodgkinson's position among the manufactories of Manchester, together with the unlimited command over the resources of one of the largest engineering establishments, which he obtained through the liberality of its proprietor Mr. Fairbairn, enabled him to direct his inquiries to the forms of pillars which are found most useful in practice. The results of his labours he has reduced to empirical formulæ, peculiarly adapted for application to the purposes of mechanical art.

Among the most useful of the practical conclusions to which he has arrived, the following are more particularly deserving of notice.

Mr. Hodgkinson has found, that in all long pillars of the same dimensions, the resistance to crushing by flexure is about three times greater when the ends of the pillars are flat, than when they are rounded. A long uniform cast iron pillar, with its ends firmly fixed, whether by means of discs or otherwise, has the same power to resist breaking as a pillar of the same diameter, and half the length, with the ends rounded, or turned so that the force would pass through the axis. The strength of a pillar with one end round and the other flat, is the arithmetical mean between that of a pillar of the same dimensions with both ends round, and one with both ends flat. Some additional strength is given to a pillar by enlarging its diameter in the middle part.

The strength of long cast iron pillars with relation to their diameter and length is also made the subject of Mr. Hodgkinson's investigations; and the result he deduces from them is, that the index of the power of the diameter, to which the strength is proportional, is 3·736. He has also determined, by a comparison of experimental results, the inverse power of the length to which the strength of the pillar is proportional. The highest value of this power he finds to be 1·914, the lowest 1·537, and the mean of all the comparisons 1·7117. He thus deduces, first, approximate empirical formulæ for the breaking weight of solid pillars, and afterwards, more correct methods of determining their strength. From experiments on hollow pillars of cast iron, formulæ representing the strength of such pillars are in like manner deduced.

The strength of pillars of wrought iron and of timber, in relation to their dimensions, is made the subject of another series of experiments. The result for wrought iron is, that the strength varies

inversely as the square of the length of the pillar, and directly as the power 3·75 of its diameter; the latter being nearly identical with the result obtained for cast iron; while in timber, the strength varies nearly as the fourth power of the side of the square forming the section of the pillar. In like manner, the power of cast iron pillars to resist long continued pressure, and the relative strengths of long pillars of cast iron, wrought iron, steel and timber, are determined.

The inquiry which constitutes the subject of this paper is not, however, the first of the kind in which Mr. Hodgkinson has been engaged; several series of experiments and papers on the strength of iron, in various forms, have been published by him at different times; and their accuracy has established his claim to our confidence on the present occasion.

Dr. Roget begged leave to observe, that, for many years past, it has been customary for the president, in his anniversary addresses to the society, to give narratives of the leading incidents in the lives, and an account of the scientific labours, of the more distinguished associates of whom death had deprived us during the preceding year. The utility of such a retrospect, he remarked, is sufficiently obvious. Consolation may be afforded to the survivors by the just tribute thus publicly paid to the memory of those they mourn. In marking the several steps of their ascent to eminence, in retracing the services they have rendered to science and to mankind, and in establishing their respective claims to our respect, our admiration, and our gratitude, fresh motives of emulation are presented to those who are following in the same arduous paths, and aspiring to the same honourable distinctions. The society can never forget how well these objects have been fulfilled by the excellent biographical notices we have been accustomed to hear from our presidents on each returning anniversary, and must feel how much reason they have to regret the omission of the usual discourse from the chair on the present occasion. It is with a view to prevent this interruption of the series being drawn into a precedent, that Dr. Roget has now been induced, by the desire of the president, to attempt supplying, however imperfectly, the omission he has alluded to. Having but little leisure to perform this task, he wished to claim the indulgence of the meeting for the many imperfections they will discover in the mode of its execution.

Of the deceased members on the home list, Dr. Roget has been able to notice only two, namely, Mr. Bauer and Sir Astley Cooper, not having received, with regard to the rest, any authentic information which was deserving of record in this place. It is impossible for him, however, to pass over in complete silence the honoured name of one, whose loss within these few days we all so deeply deplore—the late Sir Francis Chantrey.* But the calamity is too

* He was born in 1782, and expired quite suddenly on the 25th of the present month (Nov. 1841); only five days before the present meeting.

recent and too sudden to afford the opportunity, if indeed the effort could, under these painful circumstances, have been made, of collecting the materials for a narrative which might render adequate justice to his superior merits as an artist, and to his exemplary character as a man. This tribute to his memory must be reserved for a period when his biographer will be able to review the subject more extensively, and with more calm deliberation.

FRANCIS BAUER was born at Feldsberg, in Austria, on the 4th of October, 1758. While yet a boy he lost his father, who held an appointment as painter to Prince Lichtenstein; so that the care of his education devolved upon his mother. He manifested very early a talent for botanical drawing; and the first published production of his pencil, at the age of thirteen, was a figure of the *Anemone pratensis*, appended to a work of Stoerck. He came to England in the year 1788, and was about to proceed to Paris; when, on the eve of his intended departure, he was offered by Sir Joseph Banks the appointment of draughtsman at the Royal Gardens at Kew, a proposal which induced him to relinquish his intention of leaving England. He took up his residence near those gardens, and he continued to dwell, during the remainder of his life, in their neighbourhood. The salary of the new office which Mr. Bauer held was defrayed by Sir Joseph Banks during his own life, and its continuance after his decease was provided for by his will.

Mr. Bauer, in fulfilment of his engagement, made numerous drawings and sketches of the plants in the garden; and these are now preserved in the British Museum. A selection from his drawings was published in 1796, under the title of "Delineations of Exotic Plants cultivated in the Royal Gardens at Kew," containing in all thirty plates of different kinds of heaths. His drawings have also illustrated several papers published in the *Linnæan Transactions*, and particularly those of Mr. Brown. The 13th volume of that work contains a paper by Mr. Bauer on the Ergot of Rye, drawn up from materials collected between the years 1805 and 1809; and the plate which illustrates it is derived from drawings forming part of an extensive series in the British Museum, illustrating the structure of the grain, the germination, growth and developement of wheat, and the diseases of that and other Cerealia. This admirable series of drawings constitutes perhaps the most splendid and important monument of Mr. Bauer's extraordinary talents as an artist, and of his skill in microscopic investigation. The subject was suggested to him by Sir Joseph Banks, who was engaged in an inquiry into the disease of corn known by the name of *blight;* the part of Mr. Bauer's drawings which relates to that disease was published in illustration of Sir Joseph's memoir on the subject, and has been several times reprinted with it.* Mr. Bauer himself gave, in the volume of the *Philosophical Transactions* for 1823, an account of

* See *Phil. Mag.*, First Series, vol. xxi, p. 320.—EDIT.

his observations on the *Vibrio tritici* of Gleichen, with the figures relating to them; and another small portion of his illustrations of the diseases of corn has since been published by him in the *Penny Magazine* for 1833. His figures of a somewhat analogous subject, the apple-blight, and the insect producing it, accompany Sir Joseph Banks's memoir on the introduction of that disease into England, in the second volume of the *Transactions of the Horticultural Society*.

Mr. Bauer had commenced, before the close of the last century, a series of drawings of Orchideæ, and of the details of their remarkable structure, to which he made additions from time to time, as opportunities offered, nearly to the termination of his life. A selection from these, which forms one of the most beautiful and extensive series of his botanical drawings, was lithographed and published by Professor Lindley, between the years 1830 and 1838, under the title of "Illustrations of Orchidaceous Plants."

A paper by Mr. Bauer, entitled "Some experiments on the Fungi which constitute the colouring matter of the Red Snow discovered in Baffin's Bay," was published in the *Philosophical Transactions* for 1820. By mixing the snow containing these fungi with water, he found that they could be made to vegetate, but that they produced new fungi of a green instead of a red colour. By exposure to excessive cold the primitive fungi are killed, but their seed still retains vitality, and, if immersed in snow, which appears to be their native soil, they reproduce new fungi, which are generally of a red colour.

The *Philosophical Transactions* for 1823, contains the paper by Mr. Bauer already alluded to, entitled "Microscopical Observations on the Suspension of the Muscular Motions of the *Vibrio tritici*," which forms the Croonian Lecture for that year. This minute worm, which infests wheat, and is the immediate cause of that destructive disease called the *Ear Cockle* or *Purples*, congregates in immense numbers in the substance of the grains thus diseased, forming masses of a white and apparently glairy mucus, which, when immersed in water, separate and exhibit, under the microscope, the worms in lively motion. After they have become perfectly dry, and apparently lifeless, they may be readily revived by being moistened with a drop of water, when they become as lively as before, Mr. Bauer determined, by a series of experiments, that the ova of these worms are conveyed into the cavities of the germens by the circulating sap. On inserting some of the worms into sound grains of wheat, and allowing them to germinate, he found the worms, in different stages of their growth, in the stalk, and ultimately in the germens of the new plant.

In the year 1816 he commenced lending the assistance of his pencil to Sir Everard Home, in the various anatomical and physiological investigations in which the latter was engaged; and in the course of ten or twelve years furnished, in illustration of Sir Everard's numerous papers in the *Philosophical Transactions* more

than a hundred and twenty plates, which were afterwards reprinted in his "Lectures on Comparative Anatomy." These plates, which form together the most extensive series of Mr. Bauer's published works, embraced a great variety of important subjects, chiefly in microscopic anatomy, and afford abundant evidence of his powers of observation and skill in depicting the most difficult objects. It is this rare and previously almost unexampled union of the observer and the artist that has placed Mr. Bauer in the first rank of scientific draughtsmen. His paintings, as the more finished of his productions may well be termed, are no less perfect as models of artistic skill and effect, than as representations of natural objects.

He died at his residence on Kew Green, on the 11th of December last, in the 83rd year of his age.*

Sir Astley Paston Cooper, Bart., was the fourth son of the Rev. Dr. Samuel Cooper, of Yarmouth, in Norfolk. His mother was a daughter of James Bransby, Esq., of Shottisham, and was known as the authoress of a novel entitled "The Exemplary Mother." Sir Astley was born at Brooke, in the same county, on the 23rd of August, 1768. Even in his boyhood he was noted for his bold and enterprising spirit, the sociability and kindness of his disposition, and for the animation with which he entered into all the sports of his juvenile companions. After receiving from the village schoolmaster, and from his father, who was a good scholar, some portion of classical instruction, he was placed, at the age of fifteen, with Mr. Turner, a surgeon and apothecary at Yarmouth. Here he remained but a few months, and was then sent to London, and bound apprentice to his uncle, Mr. William Cooper, one of the surgeons of Guy's Hospital, but was soon after transferred, by his own desire, to Mr. Cline, who had already attained great eminence, and was surgeon of St. Thomas's Hospital. This connexion afforded him ample opportunities of acquiring professional knowledge, under the guidance of a master distinguished by a truly philosophical mind, and for whom his pupil always felt the most profound regard and veneration. Young Cooper's labours in the wide field of observation thus open to him, both in the hospital and dissecting-room, were unremitting; and the practical information he there acquired formed the solid basis of his future fame. He made a short visit to Edinburgh in 1787, and, although only in his nineteenth year, was a distinguished member of the Royal Medical Society of that place. On his return to London, Mr. Cline, who was the teacher of anatomy, physiology, and surgery, at St. Thomas's Hospital, appointed him as demonstrator of anatomy, and soon after gave up to him a part of the anatomical lectures. Sir Astley also gained the consent of Mr. Cline and the other surgeons of the hospitals of Guy and St. Thomas, to give a course of lectures on the principles

* The above account is chiefly an abridgement of that contained in the *Proceedings of the Linnæan Society* for 1841, p. 101.

and practice of surgery, a subject which had previously only formed a part of the anatomical course. He had now full scope for the display of those talents which afterwards shone forth on the wider theatre of the world, in a profession of which he became the brightest ornament. At first he was attended only by fifty students; but his class soon increased to four hundred, which was by far the largest that had been known in London. His popularity as a teacher rapidly increased: he made no attempts at displays of oratory, but always studied to render the subject which he treated as plain and intelligible as possible to his hearers, wisely avoiding distracting their attention by entering on controversial topics connected with physiology.

On the close of 1791, the year he commenced as a lecturer, he married the daughter of Thomas Cock, Esq., of Tottenham, who was a distant relation of Mr. Cline; but as a proof of his constant solicitude never to neglect the performance of any public professional duty, it is remembered that on the evening of the day on which the marriage ceremony was performed he delivered as usual his lecture, without the slightest intimation to his class of what had happened in the morning; and even at the time when he was most fully engaged in this exceedingly laborious practice, he never omitted to deliver his regular lecture at the hospital.

In 1792, after spending some months at Paris, and attending the lectures of Dessault, at the Hotel Dieu, and also those of Chopart, he commenced practice in London, taking up his residence in the city, where he dwelt for many years before he removed to the west end of the town. The popularity he enjoyed as a surgeon, and the extent of his practice, have probably surpassed that of any of his predecessors: and the large fortune which he acquired was the just and honourable reward of distinguished merit and the most unremitting application.

Sir Astley Cooper was elected a Fellow of this Society on February the 18th, 1802. He had previously contributed to the *Philosophical Transactions* two papers: the first entitled "Observations on the Effects which take place from the Destruction of the Membrana Tympani of the Ear," * and the second containing "Further Observations on the same subject; together with an Account of an Operation for the removal of a particular kind of Deafness." † The operation of puncturing the membrana tympani for the relief of that species of deafness which arises from an obstruction of the Eustachian tube, suggested itself from observing that, in several cases, an aperture in the membrane did not essentially diminish the powers of the ear, and that even its total destruction by disease is not followed by total deafness. Several cases are described in which the opera-

* *Phil. Trans.* for 1800, Part I, p. 151. [*Phil. Mag.*, First Series, vol. viii, p. 359.—Edit.]

† *Phil. Trans.* for 1801, Part II, p. 435. [Ib. vol. x, p. 86, xi, p. 268.—Ed.]

tion proved successful; but of course, when deafness proceeds from any other cause, the operation is not likely to be of the least benefit.

The other professional publications of Sir Astley are exceedingly numerous; they all bear the stamp of the peculiar character of his mind: simple and unaffected in point of style, and without pretension to elegance, they contain a plain relation of facts, unbiassed by preconceived theories, the fruits of a long and extended experience, and leading to sound practical conclusions. He never sought pecuniary advantage by his publications; and while he spared no expense in the execution of such engravings as were best calculated to afford instruction, he invariably published them at a low price.

His publications relate chiefly to the following subjects, namely: the anatomy and treatment of the various kinds of hernia; of aneurism; of spina bifida; of dislocations and fractures; of exostoses; of encysted tumours, the extraction of calculi from the bladder; the structure and diseases of the breast and of the testis. Among the last subjects to which he had particularly turned his attention was the structure and functions of the thymus gland.

The splendid anatomical and pathological museum which he had collected and created entirely by his own industry and labour, and chiefly within the last few years of his life, at a period when the ardour of most men for scientific pursuits begins to flag, consists of nearly three thousand preparations, each most exquisitely worked out, and the whole admirably arranged. The injected preparations are of unrivalled beauty, and show that he had acquired a facility and perfection in the art of anatomical injection quite peculiar to himself.

He was latterly engaged in an experimental investigation on the functions of the different parts of the brains of the lower animals. His health had suddenly declined a short time before his death, which happened on the 12th of February, 1841.

Sir Astley was left a widower in June, 1827; the year following he married the daughter of John Jones, Esq., of Derry Ormond, in Cardiganshire. He has left no children, and has bequeathed by his will the whole of his musuem to his nephew, Mr. Bransby Cooper, and he has also left some property in the funds (namely, £4,000 three per cent. consols), of which the interest is to be given as a triennial prize for the best original essay or treatise on given subjects in anatomy, physiology, or surgery, to be awarded by the physicians and surgeons of Guy's Hospital.*

Augustine Pyramus De Candolle, one of the most distinguished botanists of the present age, was born at Geneva on the 4th of February, 1778. The same year is also memorable by the death of Linnæus, the father of modern botany, which took place about

* The greater part of this memoir of Sir Astley Cooper, and especially the account of his early life, has been extracted from Pettigrew's "Medical Portrait Gallery."

three weeks before the birth of one, who was destined to emulate his fame in the same department of natural history. When seven years of age, De Candolle sustained a serious attack of hydrocephalus, a disease generally so fatal in its tendency, that the present affords a remarkable instance of complete recovery, after life had been for many days despaired of.

Possessing a remarkable facility of writing verse both in French and Latin, and having at the same time a keen relish for the study of history, young De Candolle at first resolved to make literature his profession; aspiring, as the summit of his ambition, to the fame of being a great historian. But this dream of his youth was effaced by a new taste, imbibed during a residence in the country, where he amused himself with examining the plants of the neighbourhood, and with writing their descriptions, before he had even opened a single book on botany. The few pages he there read of the volume of nature were sufficient to captivate his affections for the pursuit which henceforth became the dominant passion of his life. The botanical lectures of Professor Vaucher, which he attended in 1794, increased his ardour, and confirmed him in the resolution he had formed, of devoting himself to the cultivation of botany as his primary object, to which all other sciences, as well as branches of literature, were hereafter to be deemed subordinate, and to be followed merely as recreations from severer study.

A visit to Paris, which he made in 1795, gave him the opportunity of attending the lectures of Cuvier, Fourcroy, Vauquelin, and other distinguished professors of that period, and of forming friendships with Desfontaines and Lamarck. He always prided himself in having been the pupil of Desfontaines, in particular, towards whom he continued through life to feel the warmest gratitude and affection.

The establishment of the Society of Physics and Natural History at Geneva, which took place after his return, under the auspices of the celebrated De Saussure, gave a fresh and powerful impulse to his exertions; as was evinced by the numerous memoirs which he presented to that society.

The state of Geneva being, soon after this period, absorbed into the French empire, De Candolle was induced to quit that city and attend the medical lectures in Paris; a course of study which, tending to enlarge his views of the physiology of organized beings, contributed greatly to the success with which he afterwards cultivated the philosophy of botany. While at Paris, he founded, in conjunction with his friend, M. Benjamin Delessert, the *Société Philantropique*. One of the first advantages resulting to the public from this institution was the distribution of economical soups throughout the different quarters of the city. Of this institution he was the active secretary for ten years; during which period another society was also formed under his direction and management for the *Encouragement of National Industry*.

In 1804 he gave lectures on Vegetable Physiology at the College de France, and published an outline of his course in 1805, in the *Principes de Botanique*, prefixed to the *Flore Française*.

In 1806 he was commissioned by the French government to collect information on botany and the state of agriculture through the whole of the French empire, the limits of which, at that time, extended beyond Hamburgh to the north, and beyond Rome to the south. Every year, during the following six years, he took a long journey in the fulfilment of the task assigned him, and drew up a report of his observations for the minister. In these annual reports, however, he did not confine himself to the special objects of his commission, but made known his views with regard to the internal administrations of the countries he visited; suggesting at the same time measures for their amelioration and for the correction of existing abuses. He had projected a great work on the agricultural state of the empire, and had even executed considerable portions of it, comprehending the "French Flora," arranged according to modern views of classification, when the political events of 1814 put an entire stop to the work.

In 1807 he was appointed Professor of Medicine at Montpellier; and in 1810, a chair of botany was instituted in the same academy, which he was invited to occupy. Under his superintendence, the Botanical Garden of that city was more than doubled in extent, and the study of botany assumed a degree of importance it had never before possessed. De Candolle quitted Montpellier in 1816, very much to the regret of the students and of his colleagues, who employed every means in their power to induce him to remain among them; but his country had been restored to liberty, and he was firm in his determination to fix himself in his native city, and devote to its services the remainder of his days.

Soon after his return to Geneva he was appointed to the chair of natural history, an office which had been created expressly that he might occupy it. Among the first of the public benefits which he conferred upon his countrymen was the establishment of a botanic garden. The government of Geneva willingly lent their aid in forming so laudable an institution, in which he was also assisted by a great number of voluntary subscribers. The enthusiasm which he inspired for his favourite science was remarkably displayed on one particular occasion, when, being desirous of procuring for Geneva a copy of a Flora of Mexico, which had been deposited with him for a few days, an appeal which he made to the public was responded to with such alacrity, that in the course of eight days one thousand drawings had been finished by amateurs, who volunteered their services on the occasion.

The activity and powers of De Candolle's mind were displayed in a multitude of objects of public utility, the furtherance of which ever called forth in him the most lively interest;—whether it was the improvement of agriculture, the cultivation of the fine arts, the

advancement of public instruction, the diffusion of education, or the amelioration of the legislative code. Feeling deeply of what vast importance to the welfare of mankind it is that sound principles of political economy should be extensively promulgated and well understood by all ranks of men, De Candolle never failed to develope and enforce those principles in his lectures and popular discourses, as well as in his official agricultural reports. On these subjects, and especially with respect to the immense advantages which would accrue to the community from the unrestricted freedom of commerce, his views were those of the most enlightened policy, and exhibited a sagacity in advance of the times in which he lived.

As a lecturer, he possessed in an eminent degree the power of imparting to his auditors the enthusiasm which glowed within his own breast for the pursuits of natural history. Complete master of the subject of his discourse, his ample stores of knowledge never failed to supply him with illustrations; and even in his extempore effusions, all his ideas were developed in the clearest order, and explained with singular perspicuity.* His chief delight was to afford assistance of every kind to such students as needed it, and in whom he perceived a desire of improvement. His great aim was to inspire and diffuse a taste for the study of botany by rendering it popular among all ranks. His library, which contained the richest collection of works on that subject, and the volumes of his *hortus siccus* were always open to those who wished to consult them. Often has he been known to discontinue researches which he had commenced on finding that a similar design was entertained by another person; and he hastened, on these occasions, to communicate to this inquirer his own views on the subject, to place in his hands the materials he had collected, and to put him in possession of the fruits of his own experience. His sole object was the advance of knowledge; and whether this was effected by himself or by others was to him a matter of total indifference.

De Candolle had been visibly declining in health for some years before his end. The sudden death of Cuvier had impressed him with the apprehension that a similar fate might be impending; and that he himself might, in like manner, be cut off before he had accomplished the great works in which he was then engaged. He, in consequence, resolved to set aside all other occupations, and concentrate all his efforts in completing those more important designs. During the last year of his life he undertook, with the vain hope of improving his strength, a long journey, in the course of which he at-

* The substance of De Candolle's popular courses of lectures on the physiology of plants is contained in "Conversations on Vegetable Physiology, comprehending the Elements of Botany, with their application to Agriculture," by the accomplished authoress of "Conversations on Chemistry," "Natural Philosophy," and other well-known works. The first edition appeared in 1829.

tended the scientific meeting held at Turin, where, as might be expected, he met with the most flattering and cordial reception. His death took place on the 9th of September, 1841, in the 64th year of his age.*

SIMON L'HUILLIER, for many years professor of mathematics at Geneva, was born in that city on the 24th of April, 1750. The rapid progress which he made in his collegiate studies was viewed with so much interest by one of his relations, a minister of the reformed church of Geneva, that he bequeathed him a large portion of his fortune, on the express condition that he would embrace the clerical profession; but young L'Huillier, feeling no inclination to the studies which this condition would have imposed upon him, resisted the temptation, and preferred devoting himself to the pursuits of

* An oration by M. Rigaud, the Syndic of Geneva, pronounced at the "*Conseil Réprésentatif*," on the 27th of September, is the source which has supplied the information here given with regard to De Candolle. The following is a catalogue of such of his works as are in the library of the Royal Society:—

1. Essai sur les propriétés médicales des plantes, comparés avec leurs formes extérieures et leur classification naturelle. 8vo, Paris, 1816.

2. Regni vegetabilis systema naturale; sive ordines, genera, et species plantarum secundum methodi naturalis normas; vol. 1 et 2: 8vo, Parisiis, 1818 et 1821.

3. Théorie élémentaire de la Botanique, seconde édition, 8vo, Paris, 1819. (The first edition appeared in 1813.)

4. Prodromus systematis naturalis regni vegetabilis; sive enumeratio contracta ordinum, generum, specierumque plantarum hucusque cognitarum, juxta methodi naturalis normas digesta: partes I—IV. 8vo, Parisiis, 1824—1830.

5. Mémoire sur la famille des Légumineuses; 4to, Paris, 1825.

6. Plantes rares du Jardin de Genéve; livraisons I—III; 4to, Genéve, 1826.

7. Organographie Végétale, ou Déscription raisonnée des plantes; 2 vols. 8vo, Paris, 1827. (This work has been translated into German by Meissner, in 1828.)

8. Collection de mémoires pour servir à l'histoire du Régne Végétal: 1°. Mémoire sur la famille de Mélastomacées; 2°. Mémoire sur la famille des Crassalucées: 2 vols. 4to, Paris, 1828.

9. Mémoire sur la famille des Ombelliféres; 4to, Paris, 1829.

10. Mémoire sur la famille des Onagraires; 4to, Paris, 1829.

11. Mémoire sur la famille des Loranthacées; 4to, Paris, 1830.

12. Mémoire sur la famille des Valerianées; 4to, Paris, 1832.

13. Cours de Botanique; seconde partie. Physiologie Végétale pour servir de suite à l'Organographie Végétale, et d'introduction à la Botanique Géographique et Agricole; vol. i—iii; 8vo, Paris, 1832.

De Candolle was also the author of an essay on Geographical Botany, prefixed to the second volume of the "Flora Française" (1805), Of the article "Géographie botanique et agricole," in the "Dictionnaire d'Agriculture," published in 1809. Of the article "Géographie botanique," in the "Dictionnaire des Sciences Naturelles," 1820. And of the article "Phytographie," in the "Dictionnaire classique d'histoire naturelle."

[M. De Candolle's Memoir on the genus *Brassica* was reprinted from the *Transactions of the Horticultural Society* in *Phil. Mag.*, First Series, vol. lxi, p. 87.—ED.]

abstract science. The spirit of independence evinced by this sacrifice, together with the extraordinary aptitude he displayed for mathematical acquirements, excited the interest and conciliated the affection of another of his relations, the celebrated Le Sage, by whose instructions and counsels the most salutary influence was exercised over the studies of his pupil. Bertrand, who then occupied the chair of mathematics in the same college, was also one of those who discerned in L'Huillier the dawn of genius; and even at that early period he regarded him as destined to be his successor in that professorship.

As L'Huillier advanced to manhood, it became necessary for him to engage in some active employment, in which he could turn to account his academical attainments. He had the good fortune, at this critical time of his life, to be chosen tutor to Prince Czartorynski, with whom he remained for a period of thirteen or fourteen years; ever honoured with the friendship and respect of all the members of the Prince's family. He dedicated to the father of his pupil his first work, which was published at Warsaw, in 1782, under the title of *De relatione mutuâ capacitatis et terminorum figurarum, geometricè considerata, seu de Maximis et Minimis pars prior elementaris;* in which he treats geometrically, and with singular elegance and vigour of demonstration, all the elementary problems relating to isoperimetric figures and solids. About the same time he presented to the Academy of Berlin a memoir, which was afterwards published in its Transactions, on the minima relating to the figure of the cells of bees, a subject which he appears, in that paper, to have exhausted.*

The prize proposed by the same academy in 1786, was adjudicated to him for a memoir, which was since published under the title of *Exposition élèmentaire des principes des calculs supérieurs.* In this masterly essay the differential calculus is derived from a principle which D'Alembert had, in the first edition of the *Encyclopédie,* so happily illustrated, and which is now so generally recognised as the basis of that calculus; namely, the doctrine of limits.

On his return to Geneva in 1789, l'Huillier published an opuscle, which acquired great celebrity, entitled *La Polygonométrie; ou de la mesure des figures rectilignes, et abrégé d'isopérimétrie élémentaire, ou de la dépendance mutuelle des grandeurs et des limites des figures;* at the conclusion of which he gives a masterly summary of his former researches on elementary isoperimetry. In this work are given several formulæ of great generality, and which, at that time, were entirely new, and were calculated to facilitate the study of numerous relations arising from the perimeters and areas of polygons. About the same period, indeed, Mascheroni published formulæ very analogous to those of l'Huillier: but the latter afterwards succeeded

[* See *Phil. Mag.*, Second Series, vol. iv, p. 313.—Ed.]

in showing that he had arrived at the same results by original processes.

During the tempestuous years of the revolution, l'Huillier sought in Germany the retirement so necessary to his pursuits; and chose Tubingen as his residence. The fruit of his labours during this seclusion was a work almost wholly new, which appeared at Tubingen, in 1795, under the title *Principiorum calculi differentialis et integralis expositio elementaris.*

He was invited about this time, to the chair of the Higher Mathemathics in the University of Leyden; but his attachment to his native country was too deeply rooted to admit of his accepting this flattering offer: and eventually, in June of the same year (1795), he attained the object of his highest ambition, by receiving, after a successful public competition, the appointment of Professor of Mathematics in the Academy of Geneva.

At a subsequent period he was associated with his friend and colleague Professor Prévost in the composition of several memoirs on the calculation of probabilities, which appeared under their joint names in the memoirs of the Berlin Academy. The questions treated of in these memoirs, although they do not reach the higher problems belonging to this department of mathematics, are yet resolved by methods remarkable for their perspicuity and elegance. L'Huillier published, in 1804, his *Elèmens raisonnès d'Algèbre publiès à l'usage des étudians;* in 2 vols. 8vo, a work of considerable merit, as developing with clearness the true principles by which the understanding advances from that which is known to that which is unknown.

His last work, the *Elèmens d'Analyse Géométrique et Algèbrique appliquées à la recherche des lieux géométriques*, in 4to, appeared in the year 1809. It was dedicated to his former pupil, Prince Czartorynski, who was, at that time, minister of public instruction in the vast empire of Russia, but who has since become better known to Europe as the most illustrious of the exiled Poles.

The declining health of L'Huillier obliged him at length to resign a professorship which he had held during five-and-twenty years, and the duties of which he had ever discharged with the most undeviating regularity, and the most scrupulous exactness. Even while suffering acutely from a painful attack of sciatica, he insisted on being carried to his class, lest any detriment should arise to his pupils from an interruption to his lectures. Many of these pupils have subsequently distinguished themselves in their scientific career; among these may be cited one of our illustrious foreign members, Professor Sturm.

For the simplicity of his manners and the strict integrity of his character, L'Huillier was no less remarkable than for the vigour and extent of his mathematical powers: by these qualities he was endeared to his friends, and esteemed and respected by all, during a life protracted beyond the ordinary duration. His death occurred on the 28th of March, 1840, when he had nearly completed his 90th

year, with a constitution, however, which had some time previously been shattered and broken down by the infirmities incident to so advanced an age.*

Felix Savart, a philosopher distinguished more especially for his researches in the science of acoustics, was born on the 30th of June, 1791, at Mézières, the capital of the Department of the Ardennes, in France. He very early exhibited a decided turn for mechanical invention, and his greatest delight was to contrive and construct with his own hands musical instruments and apparatus illustrative of natural philosophy, a study of which he was passionately fond. His parents had been connected with the school of engineers at Mézières; and several of his relations having been distinguished as artists, he was himself educated with a view to the same destination. But the family afterwards removing to Metz, the path which had at first been marked out for him was abandoned, and he prepared himself for another profession, by directing his whole attention to medicine. In course of time he obtained the appointment of assistant-surgeon in the Military Hospital. Not satisfied with this situation, he, in 1814, repaired to Strasburg for the purpose of prosecuting his medical studies in the Military Hospital of that town; and he subsequently, in 1816, took a degree in medicine in the University. He then returned to his paternal roof at Metz, with the intention of settling, and of applying himself diligently to the practice of his profession. But on being restored to the scene of his youthful occupations, the renewed sight of those philosophical instruments to which so many delightful associations were attached, rekindled in full force the innate predilection for the physical sciences, which, during so long an interval, had lain dormant in his breast. The charms of science, arrayed in her most attractive colours, glittered before his imagination, and were contrasted, in his ardent mind, with the cares, the toils, and the anxieties of the profession in which he was embarking. He yielded to the powerful fascination, and disregarding all considerations of prudence, took the irrevocable step of abandoning the prospects which were opening in a career to which his youth had been devoted, and by which alone it had, till then, been his ambition to earn fortune, reputation, and independence. Confiding in his knowedge of acoustics, which was ever his favourite study, and in which he conceived he had made discoveries, he quitted his provincial domicile, and repaired to the metropolis, as to the mart where his acquisitions would be best valued. He arrived in Paris with but scanty means of immediate support, without a friend, and unprovided with a single letter of recommendation. But fortune took him by the hand, and favoured his first endeavour to obtain notice. He presented

* The above account is derived from a biographical notice by Professor De La Rive, which forms part of the *Compte rendu de l'état de l'instruction publique de Genève pendant l'année scolaire.* 1839-1840.

himself to Biot, and communicated to him his views, and the results of his researches in acoustics. He met with the kindest reception from that philosopher, who had himself been occupied with similar inquiries, and was well qualified to appreciate the merits of Savart. Biot was ever after his friend and patron, and it was chiefly through his influence that Savart was, in the year 1820, appointed Professor of Natural Philosophy in one of the institutions at Paris; an office which he continued to hold till the year 1827, when he was nominated a Member of the Academy of Sciences. Soon after this he was associated with Thénard, as Conservator of the Cabinet of Physics of the College of France. Thus raised to a state of independence, he had full leisure to devote himself to the science he had ever particularly cherished, and of which his labours have greatly extended the boundaries. His admirable researches on the laws of the vibrations of solid bodies of different forms and kinds, and in particular, of cords, of membranes, of rods, whether straight, or bent, or of an annular shape: of flat discs, and of solids of revolution, both solid and hollow, have furnished results of great value and importance. His investigation of the structure and functions of the several parts of the vocal organs, and his theory of the voice, both in man and in the lower animals, show great originality of research, and have thrown considerable light on a very difficult department of physiology.

Savart was elected, in the year 1839, a foreign member of the Royal Society, an honour which his unconquerable prejudice against the English, and everything emanating from England, prevented his ever acknowledging. His premature death, on the 16th of March, 1840, has, unfortunately for science, arrested the brilliant career of discovery, which he was pursuing with so much ardour and success, and will, it is to be feared, deprive the world of the fruits of many of his unfinished labours.*

* The materials for the above sketch were furnished by the funeral oration on Savart pronounced before the Royal Academy of Sciences of the Institute of France, by M. Becquerel, on the 18th of March, 1841.

The following is a list of memoirs by Félix Savart:—

1. Mémoire sur la construction des instrumens à cordes et à archet. Paris, 1819.
2. Mémoire sur la communication des mouvemens vibratoires entre les corps solides. (*Annales de Chimie*, tome xiv.)
3. Recherches sur les vibrations des l'air. (Ibid. t. xxiv.)
4. Mémoire sur les vibrations des corps solides considérées en général. (Ibid. t. xxv.)
5. Recherches sur les usages de la membrane du tympan et de l'oreille externe. (Ibid. t. xxvi.)
6. Nouvelles recherches sur les vibrations de l'air. (Ibid. t. xxix.)
7. Mémoire sur la voix humaine. (Ibid. t. xxx.)
8. De l'influence exercée par divers milieux sur la nombre de vibrations des corps solides. (Ibid. t. xxx.)
9. Note sur la communication des mouvemens vibratoires par les liquides (Ibid. t. xxxi.)

Description of a Thunder Storm as observed at Woolwich; with some Observations relative to the Cause of the Deflection of Electric Clouds by High Lands; and an account of the Phenomena exhibited by means of a Kite elevated during the Storm. By* W. STURGEON.

ON Saturday evening, June 14th, about eight o'clock, an electric storm passed partly over this place, exhibiting lightning the most splendid I ever beheld. The wind was pretty brisk from S. by W., about the first appearance of the storm, and if the electric clouds had obeyed the force of the wind only, the principal part of them would have come directly over us. This, however, was not the case, for instead of their being carried over Woolwich, in the direction of the wind, the most formidable group of them, and consequently the greatest fury of the storm, were deflected out of the wind's track before their arrival at Shooter's Hill, and were carried over the low lands on the other side of the hill, toward the Thames, in a direction nearly from W.S.W., to E.N.E.

The deflection of electrised clouds out of the wind's direction, though, perhaps, not much noticed, is a very common circumstance in the neihgbourhood of high lands, especially if those lands are

* From the *Phil. Mag.* Dec. 1834.

10. Mémoire sur la voix des oiseaux. (Ibid. t. xxxii.)
11. Note sur les modes de division des corps en vibration. (Ibid. t. xxxii.)
12. Note sur les sons produits dans l'expérience de M. Clement. (Ibid. t. xxxv.)
13. Recherches sur les vibrations normales. (Ibid. t. xxxvi.)
14. Mémoire sur un mouvement de rotation dont le systéme de parties vibrantes de certains corps devient le siége. (Ibid. t. xxxvi.)
15. Sur la décomposition de l'ammoniaque par les métaux. (Ibid. t. xxxvii.)
16. Recherches sur l'élasticité des corps qui cristallisent régulièrement. (Ibid. t. xl.)
17. Recherches sur la structure des métaux. (Ibid. xli.)
18. Mémoire sur la réaction de torsion des lames et des verges rigides. (Ibid. t. xli.)
19. Note sur la sensibilité de l'organe de l'ouïe. (Ibid t. xliv.)
20. Note sur la perception des sons graves. (Ibid t. xlvii.)
21. Mémoire sur la constitution des veines liquides lancées pas des orifices circulaires en minces parois. (Ibid. t. liii.)
22. Mémoire sur le choc d'une veine liquide lancée contre un plan circulaire. (Ibid t. liv.)
23. Mémoire sur le choc de deux veines liquides animées de mouvemens directement opposés. (Ibid. t. lx.)
24. Recherches sur les vibrations longitudinales. (Ibid. t. lxv.)
25. Extrait d'un mémoire sur les modes de division des plaques vibrantes. (Ibid. t. lxxiii.)
26. Note sur les causes qui déterminent le dégre d'élévation des sons (Ibid. t. lxxv.)
27. Biot et Savart.—Sur la mesure de l'action exercée à distance sur une particule de magnétisme par un fil conjunctif. (*Journal de Physique*, t. xci.)

composed of materials which are bad conductors of electricity. For although they do not absolutely refuse the transmission of the electric matter driven to the surface of the hill from the lower strata of air* by the disturbing force of the condensed electric fluid in the clouds, the transfusion into the ground is too tardily performed to prevent accumulation on the surface, which consequently becomes charged in the same state as the clouds that are approaching it. A reaction immediately takes place, and a consequent repulsion or deflection of the clouds is produced. This electric force now operating in conjunction with the wind, gives the cloud a new direction of motion, and urges it over a tract of country composed of better conductors, which are more susceptible of being transpierced by the electric matter than those from which the cloud was deflected. Hence it is that electric storms are more frequent and more violent over marshy lands, rivers, &c., than over drier and more elevated tracts of country.†

These causes operated in a very beautiful manner in giving direction to the storm on Saturday evening. The principal group of clouds, as before stated, never reached Shooter's Hill, but was carried over the low wet lands, on the other side, to the Thames; and the foremost clouds were taken to the other side of the river, and over a considerable tract of the Essex marshes. At this period another direction was given to the storm, and the new combination of forces urged it in the direction of the river, a route which electric storms visiting this neighbourhood very frequently take.

Its progress down the river was exceedingly slow, owing, as I

* The asperifolious plants, and the vegetable clothing of the land generally, especially at this season of the year, receive the electric fluid from the atmosphere in great abundance; and the myriads of vegetable points, sharp edges, &c., presented to the air, offer every facility for its reception on any emergency of pressure emanating from the repulsive force of a highly charged cloud. The surface of the land thus becomes charged at the expense of the air, each gradually resuming its natural electric equilibrium again, as the disturbing force withdraws its influence, by the progress of the cloud in its course. Thus new tracts of country become charged in succession as the cloud approaches them, and an electrical tide sweeps the face of the land by the floating influence above.

It is on this account that insulated kite-strings, exploring rods, &c., frequently become negatively electric at the approach, and during the transit, of clouds of this description. But if the kite or the exploring rod were to reach into the cloud, it is not likely that either of them would ever be found in a negative state. I am speaking of the principal influencing cloud, and not of those straggling thin patches in their vicinity, which frequently became negative by a portion of the electric matter which they before possessed being driven out of them by the predominating electric force of the superior cloud. In the same manner, an insulated metallic rod furnished with fine points or sharp edges at its further extremity, may have its natural electric

† Immense tracts of flat country, frequently become charged in the same manner and from the same cause as ranges of hills are charged; but the repulsive force from such places is directed vertically, and not so directly opposed to the horizontal direction of the cloud's motion as that proceeding from the side of a high hill.

suppose, to the wind (though much slackened before this time) being more directly opposed to it. I had been floating an electric kite in the Artillery Barrack-field during the transit of that part of the storm which passed over Woolwich. I had got completely wet with the heavy rain which fell during the time; notwithstanding which, the unusual fineness of the lightning which was playing over the river and marshes induced me to pursue it with my eye, when, from its distance, I could no longer explore the theatre of the resplendent exhibition. I walked to the top of Wellington-street, from which place I had an exceedingly fine view of the storm, now too distant to hear even a feeble murmur of those thunders, which, I am persuaded I may safely affirm, were to the inhabitants in their immediate vicinity terrible to an unusual degree.

It was now nine o'clock, and the lightning was magnificent indeed. Nature appeared as if disposed to gratify the utmost extent of curiosity by an unremitting display of her electrical elemental fire.

For about half an hour the storm appeared to be nearly stationary, hovering over a tract of the low land on the Essex and Kent sides of the Thames, perhaps not far from Purfleet. The lightning was unusually refulgent; the flashes in rapid succession, and discharged in every possible direction that can be imagined, and generally through a longer striking distance than I had ever before noticed. Three or four discharges, which occured a little after nine o'clock, darted through a horizontal arch of about 50° each; and several of those which were directed vertically and oblique to the horizon, shot through 30° or 40°, the fluid being visible in every part of the circuit.

fluid driven out of it into the air, by the approximation of a positively charged body at the other end.

I have never yet found the atmosphere *negative* with regard to the earth at any other time than when modified by such causes as I have pointed out. I have made upwards of four hundred electric kite experiments, under almost every circumstance of weather, at various times of the day and night, and in every season of the year; I have experimented on Shooter's Hill, and on the low lands on the Woolwich and Welling sides of it, and the experiments in the three different places within an hour of each other; I have done the same on Chatham lines, and in the valley on the Chatham side of them; on Norwood Hill, and in the plain at Addiscombe; also on the top of the Monument in London, and during the present year, on the top of some of the high hills in Westmoreland and in the North Riding of Yorkshire; and in every case I have found the atmosphere positive with regard to the ground. In most of these cases, the stations at the tops of the hills were higher than the place of the kite when the experiments were made at the lower ones.

I have floated three kites at the same time, at very different altitudes, and have uniformly found the highest to be positive to the other two, and the centre kite positive to that which was below it; consequently the lowest one was negative to the two above it; but still it was positive to the ground on which I was standing. I have made more than twenty experiments of this kind, and the results (with the exception of electric tension) were invariably the same; showing most decidedly that the atmosphere in its undisturbed electric state is more abundantly charged than the earth, and, as far as I have been able to explore it, still more abundantly in the upper than in the lower strata.

If this lightning was discharged over Purfleet, or thereabouts, as I have supposed, it would be about eight miles from where I was standing. Now allowing seven miles to be the mean distance of the lightning discharged in a track at right angles to the line of sight, the angle of 50° would give a chord of nearly six miles and a half for the striking distance, or the tract of air through which the lightning travelled visibly at one discharge. The apparently vertical and oblique discharges were much nearer in some part of the circuits than those which shot through the extraordinary horizontal ranges. The rain, I imagine, was falling in torrents, which would greatly facilitate the transmission through long striking distances. Moreover, the inferior density of the air in the region of the clouds, and the thin aqueous vapours which are floating there, tend very much to facilitate the transmission.

Buildings, trees, and other tall objects are not usually struck by lightning before the falling of rain, the dry dense air offering too great a resistance to be transpierced from the clouds to the ground.

From the time that the clouds arrived within the influence of the Thames, they seemed to travel nearly in its direction; for although the lightning played over some miles of country on both sides of its banks, the river appeared to be the direction line to the focus of the storm, which, if not earlier disposed of, would probably be transported by it to the Nore or to the Channel; a direction nearly at right angles to that of the wind at this place.

The lightning was very fine from the straggling clouds which passed over our heads, and from others which crossed the Thames much nearer to London. These clouds separated from the splendid group already noticed, and travelled in the direction of the wind towards the N., or N. by E., and would if not obstructed, discharge their lightnings as a distinct storm over the country about Waltham, Epping, Chipping Ongar, Harlow, &c.

The wind had abated to such a degree before I arrived in the Barrack-field, and the rain fell so heavily during the time I was there, that it was with some difficulty that I got the kite afloat and when up, its greatest altitude, I imagine, did not exceed fifty yards. The silken cord also, which had been intended for the insulator, soon became so completely wet that it was no insulator at all. Notwithstanding all these impediments being in the way, I was much gratified with the display of the electric matter issuing from the end of the string to a wire, one end of which was laid on the ground, and the other attached to the silk at about four inches distance from the reel of the kite string. An uninterrupted play of the fluid was seen over the four inches of wet silken cord, not in sparks, but in a bundle of quivering purple ramifications, producing a noise similar to that produced by springing a watchman's rattle. Very large sparks, however, were frequently seen between the lower end of the wire, which rested on the grass and the ground; and several parts of the string towards the kite, where the wire was broken, were occasionally beautifully illuminated. The noise from the string in the air was

like to the hissing of an immense flock of geese, with an occasional rattling or scraping sort of noise.

Two non-commissioned officers of the Royal Artillery were standing by me the whole of the time, who, unaware of the consequence, would gladly have approached close to the string; and it was not until I had convinced them of the danger of touching, or even coming near to it, at a time when the lightning was playing about us in every direction, that I could dissuade them from gratifying their curiosity too far; probably at the expense of their lives. We anxiously and stedfastly watched what was going on at the end of the string, and the display was beautiful beyond description. The reel was occasionally enveloped in a blaze of purple aborized electric fire, whose numberless branches ramified over the silken cord, and through the air to the blades of grass, which also became luminous on their points and edges, over a surface of some yards in circumference. We also saw a complete globe of fire pass over the silken cord between the wire and reel of the kite-string. The soldiers thought it about the size of a musket ball. It was exceedingly brilliant, and was the only one that we noticed.

I had no electrometer with me, nor any apparatus with which I perform chemical and magnetical experiments by atmospheric electricity; hence the whole of the time on this occasion was devoted to mere observation.

The following is a notice, extracted from the *Lancaster Gazette* of the 5th of April last, of some experiments made with an electrical kite at Kirby Lonsdale, in Westmoreland:—

On Saturday the 29th of March, I had a very favourable opportunity of demonstrating experimentally to several of my friends at Kirby Lonsdale, who had attended my recent course of lectures at that place that an abundance of the electric fluid usually attends hail and snow storms. The wind was pretty brisk, cold, and from the W. by N. nearly the whole of the day. There were several hail showers, each of which, with a simultaneous increase of wind, became a complete transient storm.

During three of these hail storms, I floated one of my silken electric kites, with a wired string of about 300 yards long, and insulated in the usual way by means of a silken cord.

The kite was elevated in each experiment about ten minutes prior to the arrival of the hail storm, and the electric state of the atmosphere ascertained, which was found to be so exceedingly feeble that not the slightest spark could be observed. As, however, the cloud from which the hail was falling approached the kite, the fluid from the string presented itself in brilliant sparks to the knuckle; and during the transit of the cloud, became so abundantly discharged to a wire presented to the string, that it struck in rapid succession through a stratum of six inches of air; and through three inches of air, it presented a splendid continuous stream of electric fire. As the cloud receded from the kite, by advancing in its aërial course, the electric discharges became less and less brilliant, and continued to

diminish in splendour and energy with the recession of the passing storm, ultimately vanishing altogether by the emergence of the kite from the electric influence of the cloud.

These appearances were exhibited in each experiment, but the display of the electric fire was the most magnificent in the second, which was the fiercest hail-storm of that day, and happened between two and three in the afternoon. During an early part of this storm the electric fluid made a continuous rattling noise down the kite-string (in consequence of the wire being broken in several places), and darted from the reel at the inferior extremity, to greater distances than in either of the other experiments. In one instance it struck over a stick a yard long, to the hand of a young man named Croft, who was presenting it to the kite string. Although the remote end of the stick was in connexion with the ground by means of a very wet string, and consequently a considerable discharge must, at the same time, have passed down the wet string to the earth, the shock was so violent as to make Mr. Croft reel and nearly fall; and I have some reason to suppose that it has left an impression on his memory, which time will not speedily obliterate. The kite-strings, however, broke soon afterwards, and consequently the experiments, on that occasion, terminated very abruptly, and unfortunately at a time also when the fire was streaming from the string in the greatest abundance, and with a degree of splendour better imagined than described.

During the third time that the kite was afloat, about two hours after the former, several gentlemen present experienced smart electric shocks direct from the kite-string.

Artillery Place, Woolwich,
June, 16, 1834.

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THE ANNALS

OF

ELECTRICITY, MAGNETISM,

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CHEMISTRY;

AND

GUARDIAN OF EXPERIMENTAL SCIENCE.

SEPTEMBER, 1842.

The Study of the Phenomena which are produced by Currents discontinued and transmitted in contrary directions, when they traverse a Circuit formed of Metallic and Liquid Conductors; preceded by Preliminary Researches on the Oxidation of Platinum.

(Continued from page 98.)

PART SECOND.

In renewing the study of the phenomena which have formed the subject of my preceding memoir, I perceived that they almost all had their source in the peculiar chemical effects which the currents made use of produced by induction. These currents owed to the circumstance of their being transmitted alternately in contrary directions, the fact of bringing successively upon the metallic conductors which conveyed them into a liquid, the opposite elements which compose this liquid—oxygen and hydrogen, for example, if it is water. Now, this rapidity with which these contrary elements succeed each other on the same metallic surface, determine in them chemical effects of a remarkable order. The result is, that the relative part which the metallic and liquid conductors play in a circuit so composed, is quite different accordingly as the currents which this circuit transmits are continued, or are transmitted alternately in contrary directions. It appeared to me that I was bound to seek in what this difference really consisted, and that with these causes, I ought to commence my work: it is, then, to this study that I devote this second part of my memoir, reserving to myself the task of developing at a future time its applications and consequences.

But previous to advancing directly to the particular point of the researches which I have just referred to, I shall treat on a question which is intimately connected, and the previous solution of which is of great importance to the following part of my work. I mean to speak of the property which platinum has of becoming oxidized under

the immediate action of oxygen. I had already suspected the existence of this property from several phenomena which I have referred to in my former memoir. But I have always felt that before regarding it as completely demonstrated, and capable of being admitted as the basis of interpretation of so many results, that it ought to be supported on proofs drawn from direct experiment. This is what I have endeavoured to do, and it is the result of this work that I am about to detail in the first section of this second part of my memoir. Perhaps it will be found, at first glimpse, that this question is in some degree strange to the principal subject of my memoir; but, as will be shown in the second section, it is so little so, that the greater part of the phenomena which are described in this section are more or less completely attached to it. Besides, the subject of the oxidation of platinum itself, and the consequences which may be drawn from it in different points of view, appeared to me to present sufficient interest to justify me in supposing that, even if it was a digression, and that of some extent, it was one which I dare not forego, seeing that the question was not treated of in a manner sufficiently complete to form a memoir alone.

FIRST SECTION.

I.—*Preliminary Researches on the Oxidation of Platinum.*

I have attributed, in my preceding memoir, the formation of the pulverulent coating with which the surface of the platinum wires becomes covered, when they are used to transmit, during a certain time into a liquid conductor, the magneto-electric current, to the oxidation, and to the successive reduction which the surface of the metal undergoes, by the alternate disengagement of oxygen and hydrogen, which proceeds from the decomposition of the water, operated upon by these currents. I have likewise admitted the idea, that if we have not recognised sooner the faculty which platinum possesses of becoming oxidized, it is because we have not distinguished the simple superficial oxidation, which is the only one of which the metals called *non-oxidable* are susceptible, from the oxidation of which the *oxidable* metals are susceptible: an oxidation which penetrates more or less profoundly beneath their surface, as by the effect of a real cementation. I will give, in a few words, the motives which have led me to draw these consequences from the fact which I have just referred to, in order to discover the formation of the pulverulent coating.

1st. The phenomenon takes place exactly in the same manner with the metals which are well known to be oxidable, such as silver and copper; only the formation of the pulverulent coating is more rapid on these latter metals, and the disengagement of gas which takes place at the commencement ceases almost immediately, whilst that with wires of gold, and still more so that with platinum, is of considerable duration.

2nd. There is, however, a moment in which the disengagement of

gas ceases also with the gold and platinum wires; and it ceases when, even as in the case of the other metals, the surface having become very much divided, oxidation, and, consequently, the reduction of the metals, is greatly facilitated; but it requires a longer time with platinum than for the other metals to arrive at the point where the gases are no longer visible.

3rd. When we continue the transmission of magneto-electric currents, even when the disengagement of gas no longer takes place, we perceive a rapid succession of changes of colour on the surface of the gold or platinum wires, similar to those which are seen on the surface of wires of oxidable metals, and particularly silver, in the same circumstances. The changes of colour correspond exactly to the alternate disengagement of oxygen and hydrogen produced by the currents, and, consequently, to the alternate oxidation and reduction of the metal.

The motives which precede, and which I have drawn from experiments contained in my preceding memoir, did not yet appear to me, as I have already stated, sufficient to warrant the admission as a principle acquired to science—the fact of the oxidation of platinum under the immediate action of oxygen. I then took up the subject anew, with the view of studying it more closely, so as to prove in a decisive manner this new property, and to follow up some of its consequences.

I sought at first to show that platinum may become oxidized and deoxidized by the immediate action of oxygen and hydrogen disengaged at positive and negative poles of a pile. I shall show in the sequel that it is in the oxidation of which platinum is susceptible, that we may find the cause of electric currents which are produced in many cases by a pair of plates both of which are platinum; in fine, I shall signalize some phenomena of another order, which seem to me to depend on the property of platinum to become oxidized, and, consequently, furnish new proofs of the existence of this property.

II.—*Action on Platinum of Oxygen and Hydrogen disengaged at the Positive and Negative Poles of a Pile.*

In order to prove directly the oxidation of platinum, I endeavoured to expose a large surface to the action of the nascent oxygen, by making use of the positive pole of a pile in acidulated water, and then transporting it afterwards to the negative pole. I took for the second pole a very fine wire, very short, and likewise of platinum, in order to establish the greatest difference possible between the two surfaces. I collected with care, and separately in graduated tubes, the gas disengaged at each of the poles, and when the relation between the quantities of oxygen and hydrogen was not exactly that which constitutes water, I concluded that the gas which was not found in sufficient quantity for the proportion necessary, had been employed to oxidize or to deoxidize the great surface of platinum, accordingly as it was the oxygen or hydrogen that was in minority.*

* We should rather suppose that the *excess* of one or the other of these gases is intended to be understood.—Edit.

These experiments have been made successively with nitric acid and with sulphuric acid, diluted with nine parts of water in volume; the two acids were perfectly pure as was the water likewise. The results have constantly appeared to me more conclusive with the solution of sulphuric acid; because in this solution, the water alone was decomposed, whilst in the nitric acid solution, a part of the acid itself being decomposed, the gases disengaged were not uniformly oxygen and hydrogen; there was found a little of the oxide of azote, and even pure azote. This inconvenience was afterwards avoided by employing nitric acid extremely diluted, but it then became necessary to have a very strong pile.

After having well cleansed a plate of platinum* of twenty centimetres in length by three in breadth, and washed it in distilled water, I rolled it into a spiral, and plunged it into water acidulated with sulphuric acid, under a graduated tube filled with the same liquid; a very fine and short platinum wire was plunged into the same acidulated water under a tube equally graduated. We could make the plate and the wire communicate with the poles of a pile by means of a platinum wire which was enclosed in tubes of glass, in such a manner that the gases proceeding from the decomposition of the liquid were entirely received in each of the graduated tubes or measures. The plate was first brought into communication with the pole —, and the wire with the pole +: exactly 100 centimetres of hydrogen were obtained at the plate, and fifty of oxygen at the wire. The poles were changed: there was then sixteen cubic centimetres of oxygen at the plate, and forty-one of hydrogen at the wire; there was wanting then four and a half cubic centimetres of oxygen, which had been taken by the plate. This oxygen had not the power of being employed in combining with the hydrogen which should have been retained by the plate when it was in connexion with the negative pole. In fact, since hydrogen was not wanting in the former experiment, it did not remain adhering to the plate. Beside, in this experiment, as in the following, care was always taken to shake the plates very much, to detach all the gas from them which might remain adhering to their surface.

In another experiment, after carefully cleansing the plate as always before observed, it was first put in communication with the + pole, and the wire with the — pole: eight cubic centimetres of oxygen were obtained at the plate, and twenty of hydrogen at the wire; thus there was wanting two cubic centimetres of oxygen. The poles were reversed, and there was now obtained at the wire ten cubic centimetres of oxygen, and fifteen or sixteen of hydrogen at the plate; there was wanting from four to five cubic centimetres of hydrogen, almost double, and consequently the equivalent of the oxygen, which

* In cleansing plates of platinum, I have constantly followed the process indicated by Farady, which consists in heating them to redness, and rubbing them with a piece of potash whilst in a state of incandescence, then plunging them in boiling sulphuric acid, and afterwards washing them for a long time in distilled water frequently renewed.

had disappeared, and which had been employed to oxidize the of surface the plate.

I made the same experiments, making use of water very slightly acidulated with nitric acid for my liquid. The plate of platinum was cleansed as before, with much care, and rolled into a spiral; the platinum wire was only from four to five millimetres in length. Having brought the negative pole into connexion with the wire and the positive pole with the plate, I obtained eighty-seven cubic centimetres of hydrogen at the wire, and only forty of oxygen at the plate; there wanted then three and a half cubic centimetres of oxygen. I left the apparatus for twenty-four hours without any change taking place, the plate and the wire soaking in the liquid. I now brought the + pole anew into connection with the plate, and the — pole to the wire: I obtained exactly forty cubic centimetres of oxygen at the plate, and eighty of hydrogen at the wire; a proof that the coat of oxide formed on the surface of the platinum the day previous had continued, in spite of the contact of the slightly acidulated water. But having put the — pole to the plate, and the + pole to the wire, I had fifty cubic centimetres of hydrogen at the plate, and twenty-nine of oxygen at the wire; there was therefore wanting eight cubic centimetres of hydrogen, almost the equivalent of oxygen, which had rested on the plate. I took care, in each of these experiments, to change that part of the liquid which was in the measure *(eprouvette)*, near the plate and the wire, in order that the conditions of the experiment might be the same in each case; nevertheless, without exposing the plate and the wire to contact with the air.

I will now detail some experiments with water, holding in solution one-tenth of sulphuric acid by volume.

With a very feeble pile the + pole was connected with the plate, and the — pole to the wire; I obtained twenty measures of hydrogen, and only six of oxygen; there was wanting then four. I took a very powerful pile, the + pole of which was brought into contact with the wire, and the — pole in communication with the plate. Five measures of oxygen and six of hydrogen were now obtained; there was wanting of course four measures of hydrogen, which proves that there had only been two measures of oxygen taken, or adhered to, by the plate, in the portion of which the hydrogen at least had produced the reduction. The poles were again brought into the same position, and there was eight measures of oxygen at the wire, and sixteen of hydrogen at the plate. A plate of platinum which had remained exposed to the air was then taken, and being brought to the — pole, whilst the + pole communicated with the short wire, we only obtained five measures of hydrogen against five of oxygen; there then wanted five measures of hydrogen, which had been employed in reducing the slightly oxidized surface of the platinum.

A plate well cleansed was put to the — pole, and the wire was placed at the + pole: there was an almost inappreciable small quantity of hydrogen deficient. The poles were changed: there was

at first ten of hydrogen, and three of oxygen only: after a second and similar operation, there was twenty of hydrogen and nine of oxygen, in all thirty of hydrogen and twelve of oxygen; there was wanting then three of oxygen. The experiment was continued, changing the poles from time to time, and there was wanting in all cases some measures of those gases which were disengaged on the plate; but this disappearance no longer took place when the plate had already been used for some time in the disengagement of the same gas.

I ought to remark, that when I alternately applied the + pole and the — pole on the plate, the quantity of oxygen and the quantity of hydrogen which disappeared was not always an equivalent to the other. I attribute these differences, on one part, to the fact, that the liquid dissolved a little of the oxygen, on the other hand, to the circumstance, that the current is not uniformly distributed on all the points of a plate, especially when the surface is great; and consequently it may happen that all the points of the plate which have been covered by one of the gases, are not exactly covered again by the other. Perhaps also a part of the oxide formed becomes dissolved in the acidulated water, which explains why it is that there is always proportionally more oxygen than hydrogen disappearing. I shall give farther hints on another cause of this difference.

I have repeated the same experiments, substituting successively for the plate of platinum, spongy platinum and a wire of platinum, the surfaces of which had been rendered pulverulent by means of the alternate oxidations and reductions of the magneto-electrical currents to which it had been submitted. With the spongy platinum, which had been soaking several days in the acid, and had been purified, and which I placed at the negative pole, I obtained the desired proportions of the two gases. But having put it now in communication with the positive pole, I had only three measures of oxygen against ten of hydrogen. The remaining phenomena which were exhibited were the same as with the plate of platinum; though the morsel of sponge was very small, the very great number of points of contact which it presented to the liquid, from the fact of its nature, rendered it still more sensible than the plate to the successive absorptions of oxygen and hydrogen due to the alternate oxidations and reductions of the metal.

The wire, the surface of which was pulverulent, was about twelve centimetres long. Having been put in contact with the — pole, immediately after the operation which had thus modified its surface, an operation from which it always becomes a little oxidized, as I shall show farther on, it only gave five measures of hydrogen against five of oxygen, disengaged at the positive pole, which communicated with the ordinary platinum wire of five millimetres in length. Having changed the position of the poles, after this first operation, there were two measures of oxygen against eight of hydrogen; there still wanted two measures of oxygen; in all five, between the two experiments. Then, from thence the gases were disengaged in their proper proportions. Having once more changed the poles, I had only

twelve of hydrogen at the long wire, against ten of oxygen at the short one; there was now eight measures of hydrogen minus, employed to reduce the oxide formed by the oxygen, which had been wanting in the preceding experiments. Having again made a last experiment without changing the poles, I obtained exactly ten measures of hydrogen from the long wire against five of oxygen at the shorter one.

Finally, I placed in the same eudiometre, always filled with acidulated water, a long platinum wire, whose surface was pulverulent, and a short one, whose surface was smooth. I brought the long wire into communication with the + pole, and the short one with the — pole; then when the gaseous mixture had filled the moiety of the eudiometre, I exploded it, and there remained an excess of hydrogen equal to a tenth of the whole volume. Having repeated the same experiment three or four times, I obtained almost constantly the same result; but at the close, the metallic powder which covered the long platinum wire, had almost totally disappeared. It is probable that the part of this very fine powder which became oxidized, was dissolved in part in the acid, and thus laid bare the new particles, which, in their turn, became oxidized. This is what explains how it is that there was constantly an excess of hydrogen in the gaseous mixture. The same phenomenon was not exhibited with the plate, whose surface, which was not pulverulent, had not the power, even when it was oxidized, to dissolve in the acidulated water. It did not take place, either, with a long wire with a bright surface rolled helically, which I substituted for the wire with the pulverulent surface. In the latter case, the gaseous mixture which I obtained by putting the helical wire in communication with the + pole of the pile, presented an excess of three measures of hydrogen. By changing the position of the poles, and leaving the residual hydrogen in the eudiometer, I had again a gaseous mixture, which I detonated, but it presented scarcely any residue; the excess of hydrogen proceeding from the first operation even, had totally disappeared. The excess was, then, due to this: that a part of the oxygen had been employed in oxidizing the surface of the long platinum wire, when that wire communicated with the positive pole; it disappeared because that, in making this same wire communicate with the negative pole, a part of the hydrogen which ought to have been found in the gaseous mixture, had been employed to deoxidize the wire.

The result of the preceding experiments appears to me to be:—

1st. That platinum perfectly pure and cleansed may be oxidized at its surface under the action of the nascent oxygen.

2nd. That this oxide is not dissolved in water slightly acidulated, when the surface of the platinum on which it is formed is not pulverulent.*

* If the acid is concentrated or boiling, it is no longer the same. The plate which has been exposed to the action of an acid in these conditions, and which has afterwards been washed in distilled water, conducts itself as a perfectly unoxidized plate.

3rd. That this oxide is very easily reducible by the action of hydrogen.

There may perhaps be two objections raised against the conclusions I have made: the first, that these phenomena which I have described may be due to the solution of the gases in the liquids; the second, that they are only the result of a simple adherence, sometimes of oxygen, sometimes of hydrogen, at the surface of the platinum.

We will now examine successively these two objections. I ought to acknowledge that the acidulated water may dissolve a part of the gases which are disengaged at each of the poles of the pile, and more particularly a little oxygen: it must also be admitted that the gas, by being developed on a large surface, facilitates this solution. But the whole of the phenomena cannot be thus explained. When after having put the platinum plate in contact with the positive pole, and having observed the disappearance of a certain quantity of oxygen, I repeated the same experiment, after taking great care to completely renew that part of the liquid which was in contact with the poles, I found that the oxygen disengaged very nearly the requisite proportion. If the absence of a part of the oxygen in the former case was due to its solution in the ambient liquid, how is it that this solution and consequently this same absence did not take place in the second case in the same manner? How is it that a platinum plate put to the — pole, after having been cleansed and washed, gives hydrogen in the requisite proportion, whilst the same plate after having been exposed to the action of oxygen, causes a portion of the hydrogen which is disengaged at its surface, to disappear in the same liquid?

The following observation, which is very easy, shows very well that it is impossible to attribute these phenomena which we have just described, at least totally, to the absorption of the gases by the liquid. When we take a plate of platinum which has been exposed for some time to the air, or to the action of oxygen, and which we plunge into a liquid conductor, making use of it as the negative pole of a pile, the current from which traverses this liquid, some seconds, sometimes up to twenty, elapse, before the hydrogen makes its appearance, whilst the oxygen is disengaged immediately on a similar plate placed at the positive pole. When, on the contrary, we put the poles in communication with two plates of platinum perfectly cleansed, the hydrogen appears on the negative plate, whilst the oxygen does not show itself until some seconds later on the positive plate. It is then evident that the differences in the precise periods when the gases become visible on each plate, are due even to the state of the surfaces of the plates, and not to the faculty which the liquid has for dissolving the gas, a faculty which should have acted in the same manner in the experiments which we have just reported.

In fine, the secondary polarites which the platinum plates acquire, polarities whose true natures vary according as the plates have disen-

gaged hydrogen or oxygen at their surface, are a further proof that this surface has been modified by these gases, and that it has been so we shall see instantly, by the effect due to an oxidation or a reduction.

Here the second objection presents itself. The platinum plates retain some oxygen at their surface it is said: but this gas is only adherent, it does not form any chemical combination, and it is the same with the hydrogen, which also adheres in its form to the surface of the platinum. These phenomena are due to an action purely physical, that of adhesion; and it is added, that when the plate which has been in communication with the positive pole is transported to the negative pole, the disappearance of a part of the hydrogen is due to this: that in its nascent state it is combined with the oxygen which remains adhering to this plate.

I have likewise examined with care this part of the question, the more so as the opinion which I have just developed is the same as that adopted by M. Matteucci, and, as it appears to me, by M. Schoenbein, in the experiments which these two philosophers have made, each on his own part, on the second polarities.

These are, now, the motives which have led me to believe that it is not a simple adhesion, but a true chemical combination between the gases and the surface of the platinum:—

1st. When the surface of the platinum is newly cleansed and scoured with care, the oxygen alone adheres, and not the hydrogen. If, in putting a plate of platinum in a tube *(eprouvette)* full of hydrogen, M. Matteucci saw the volume of the gas diminish, it was because this plate had had its surface slightly oxidized, by its exposure to the air or the direct action of oxygen. The plate, in fact, when it has been deoxidized and washed, takes up no hydrogen, whether it is placed at the negative pole of a pile, or put into a tube filled with hydrogen.

2nd. The pulverulent state which a surface of platinum takes, when it has been exposed during a long time to the alternative action of oxygen and hydrogen, is a proof that the adhesion of the oxygen has been a true chemical combination, and that of the hydrogen a veritable decomposition of the oxide. For the same thing takes place with platinum exactly as with other metals, such as copper, for instance, when their surface undergoes a succession of oxidations and reductions, this surface becomes pulverulent.

3rd. It is impossible to perceive, even by the aid of a powerful magnifier, the least bubble of gaseous oxygen on the surface of the platinum which has been exposed to the action of this gas. The friction of a linen cloth will no longer remove this oxygen; it requires, in order to effect its disappearance, either a mechanical action which will renew the surface, or a strong and boiling acid which will dissolve the oxygen formed. This is, then, another proof that it is a true chemical combination: it is true that it is only superficial, and if we esteem this circumstance as sufficient to render the

phenomenon, then the question is no more than a question of words. But it is not thus, it seems to me, that it can be understood: a simple physical adhesion implies the idea that the adhering bodies preserve their own individual properties. Now, it is not so in this case, since the oxygen is no longer gaseous, and the surface of the platinum finishes by becoming reduced into a powder by the successive action of the gases: thus the phenomenon has all the characters of a veritable chemical phenomenon.

The experiments which I have just reported were made in the spring and summer of the year 1838. They formed the subject of a memoir which I read to the Society of Physical and Natural History of Genoa, the 4th of September of the same year; I did not then print this Memoir, but I addressed an extract of it to M. Becquerel, which he communicated to the Academy of Sciences at Paris, and it was published in the *Compte Rendu* of the Sittings of that Academy, December, 1838.

I have repeated frequently, since that time, the experiments I have just described, and they have constantly given me the same results. Still more recently (the 28th of April, 1841), I obtained the following result, with a pile of constant force, consisting of ten elements, the poles of which communicated with a plate of platinum, formed into a helix, and which had been perfectly cleansed, (*decapée*) and also with a very short platinum wire; the plate and wire were immersed in sulphuric acid carefully purified, and diluted with nine times its volume of distilled water; the gases disengaged from each of the poles were received in graduated tubes. At the commencement, the — pole being brought into communication with the plate, the gases were in their right proportions. The plate was now brought to the + pole, the wire to the — pole; there were twenty centimetres of hydrogen and only six of oxygen in place of ten. On changing the poles anew, I obtained ten centimetres of oxygen and only fifteen of hydrogen; there ought to have been wanting eight in place of four of it, to represent the equivalent of oxygen which had disappeared. But, as I have already said, it is very rarely that there disappears in these experiments a quantity of hydrogen equivalent to that of oxygen. I will return to this case further on, in indicating one of the courses of which I have not yet spoken, which may contribute to occasioning this difference between the two gases.

SECTION II.

On the Influence which the Oxidation of Platinum exerts on the production of Electric Currents, by Couples composed uniquely of Metals.

I am the first, I believe, who has shown that when two platinum wires have served to decompose water by the effect of a voltaic current, these two wires may form a voltaic pair which gives birth to a current from the wire which has served at the positive pole; and at

which, consequently, oxygen has been disengaged, is the negative element of the pair; the wire used at the negative pole, and at the place where the hydrogen was disengaged, is the positive element. This current lasts for some instants, and ceases more or less quickly according to the nature of the liquid in which the two wires were immersed during the decomposition of water produced by the current, and according to the nature of the liquid into which they are afterwards plunged to form the pair.

I have attributed this phenomenon, in the memoir in which I have described and studied it,* to a particular electrical state into which the molecules of the metallic wires were constituted, during the time that they transmitted the current, and I have supported my hypothesis on the fact that those parts of the platinum wires which are not immersed in the liquid while the current passed, manifest, though in a less degree, the same properties as those corresponding portions of the wires on which were deposited the elements of the liquid decomposed. M. Becquerel, at a later period, has shown that this explanation was not the true one, and that the effect observed was due to an excessively thin deposit of the elements of the substances decomposed by the current, which deposit takes place on the surface of the platinum wires. When the current ceases passing, the action of the liquid on this deposit determines the current called secondary, in which the platinum wires play the part, the one of a positive element, and the other that of a negative element. Subsequent experiments, however, have proved to me that, even as I have already advanced, the wires and plates of platinum which have not received this deposit on their surface, may give rise to a secondary current, much more feeble, it is true, but still perceptible. I will only add that the most profound study that I have made of these phenomena, have led me to believe that they are entirely due to a chemical effect of the liquid on the surface of the platinum, not only in the case where there is a deposit, as M. Becquerel has indicated, but also in the case where the deposit has not taken place.

MM. Matteucci, Peltier, and Schoenbein, who have been occupied in examining this subject, have shown that the plates of platinum and of gold are capable of developing a secondary current, by the single fact of their having served in the decomposition of water slightly acidulated, and even of water perfectly pure, without the products of the decomposition having been any other than oxygen on one of the plates, and hydrogen on the other. M. Matteucci attributes this phenomenon to the adherence of the oxygen and hydrogen on each of the plates where these gases are disengaged. He proves this adherence by introducing the plate to which oxygen is conveyed into a graduated measure filled with hydrogen, and that on which hydrogen has been disengaged into a graduated measure

* *Mem. de la Soc. Phys. et d'Hist. Nat. de Geneve*, t. iii (2 partie), et *Ann. de Ch. et de Phys.*, t. xxxvi, p. 34.

filled with oxygen, and by proving that the volume of the gases is diminished a little in each measure, he has succeeded in producing the same secondary polarities, by placing for some moments the plates of platinum, the one in an atmosphere of oxygen, the other in an atmosphere of hydrogen, before plunging them into the liquid conductor, which ought to form the circuit, in place of putting them to the positive and negative poles of the pile.*

In M. Peltier's† estimation, the effect is due to the gases which remain dissolved in the water; and he has succeeded in producing a secondary current, by causing the hydrogen to pass directly into the water, and by putting on one of the extremities of the platinum a galvanometer into this hydrogenated water, and the other extremity likewise of platinum in ordinary water, in contact with the former.

Lastly, M. Schoenbein‡ has made a great number of experiments by immersing platinum wires, or gold and silver ones, in different gases, and determining the polarities which these wires acquired or lost by the action of these gases; he likewise employed different liquid conductors to complete the circuit of the pair formed by the metallic wires, and has studied the influence of these liquids. He appears to attribute all the effects which he obtained to chemical action, without well explaining these actions, for he adds: *I dare not yet say that these secondary currents are entirely due to an ordinary chemical action.*

The conclusions at which I have arrived at the close of the researches contained in the paragraph preceding, have made me presume that it is possible that the oxidation and deoxidation of platinum may perform an important part in the production of secondary currents. The experiments of the philosophers above named agree with the explanation, and I have sought to verify it by studying with care the phenomenon in question. I felt assured from the first that wires and plates of platinum which have served in the decomposition of water slightly acidulated, give birth to a secondary current extremely strong, and stronger in proportion as the surfaces are greater. The most decided intensity was obtained by making use of a piece of spongy platinum. A wire whose surface had been rendered pulverulent by magneto-electric currents, also gives a secondary current much more intense than a wire whose surface is united, probably because it presents, like the sponge, a much greater number of points of contact with the ambient medium.

This, in my opinion, is the cause of the phenomenon: the platinum which has been exposed to the action of hydrogen being perfectly cleansed, is attacked slightly by the acidulated water, and even by the air dissolved in the water when it is plunged in pure water; it then gives birth to a current when we form a pair with it, and another morsel of platinum whose surface has been oxidized,

* *Bibl. Univ.*, t. xvii (October, 1838), p. 378.
† *Bibl. Univ.*, t. xviii (November, 1838), p. 186.
‡ *Bibl. Univ.*, t. xviii (November, 1838), p. 187.

whether by its exposure to the air, or what is still better, because of its having been in communication with the positive pole; in the second case, the current is stronger than in the former one. This current is stronger and of longer duration with a plate that has a large surface, with spungy platinum, or with a wire whose surface is pulverulent. In these cases the chemical action takes place on a greater number of points, and as it should not cease until the surface is oxidized, it thus necessarily ceases more tardily. It is probable also that the divided state of the platinum permits a slight dissolution of the oxide, and thus constantly presents to the chemical action a portion of surface unoxidized. The explanation I have just given is a consequence of the oxidation of which I have shown that platinum is susceptible: let us now see if it is in accordance with facts.

I soaked for a long time in concentrated nitric acid, wires, plates, and spongy platinum, after having well cleansed them; I then washed them carefully in distilled water, which I renewed several times, in order that there should be no trace of acid left; I always took care that the surfaces of the metals were not in contact with the air. I fixed to the extremities of a galvanometer first a wire and a plate: the plate was positive in relation to the wire; afterwards a wire and a plate on one side, and spongy platinum on the other: the sponge was constantly positive in relation to the plate and the wire; it was the same with a wire whose surface was pulverulent, but this wire was negative with regard to the sponge. The liquid in which the metals were immersed was water, slightly acidulated with nitric acid, or sulphuric acid, both perfectly pure. These experiments seem to indicate perfectly a chemical action on the cleansed surfaces of platinum, which gives birth to a current in which the greatest surface, which is that on which the most extensive chemical action takes place, is positive in relation to the other, as that passes for the oxidable metals.

I afterwards took two platinum plates, which, after having been cleansed and washed with the greatest care, were left immersed in distilled water; I then dried it in the air, and the other under a bell receiver, which I exhausted of air as quickly as possible, and in which I had placed a capsule filled with concentrated sulphuric acid, in order to hasten the dessication. The plate that had been dried in vacuo was constantly positive to that which had been dried in the air, when they were both immersed in nitric or sulphuric acid diluted with water; a proof that the latter had been oxidized at the surface, and not the former, or that at least this one had been oxidized in the least degree of the two.

I also dried a plate of platinum which had been cleansed in hydrogen: it was positive, like that which had been dried in vacuo, but more strongly so; which is not astonishing, seeing that all trace of oxidation should disappear in this case on the surface of the platinum by the effect of the hydrogen.

By immersing two other plates in concentrated nitric acid, I found

the reverse: that is to say, that the one that had been dried in vacuo, or in hydrogen, was negative with regard to that which had been dried in the air. I therefore conclude that in this case, the plate oxidized by its exposition to the air, was attacked by nitric acid, which combined with the oxide of platinum by dissolving it, whilst that the other plate, none or very little oxidized, was not affected or very little so, by the acid; the direction of the current was perfectly in agreement with this explanation.

In order to assure myself of its correctness, I made the following experiments; I left soaking during some months, a piece of spongy platinum in very pure concentrated nitric acid; I then put it in nitric acid very much diluted, and left it in a capsule exposed to the air, but covered with paper, in order to preserve the surface of the platinum from the dust. A large platinum capsule proper for the purpose, the interior of which, however, had been for a long time in contact with the air, was put in communication with the extremity of a galvanometer; it was filled with nitric acid, pure and concentrated; I then immersed in the acid, after having put it in communication with the other extremity of the galvanometer, by means of a platinum wire. at first a morsel of sponge which had been well deoxidized by its long continuance in nitric acid, then a morsel of sponge which, after having been deoxidized, had been left immersed in water, or exposed to the air; the first morsel was negative, the other two strongly positive, particularly that which had been exposed to the air.

There was then put into the capsule nitric acid very much diluted in place of the concentrated acid; the results were precisely inverse. This difference is explained very well if we remark that the action of nitric acid, pure and concentrated, does not take place on platinum not oxidized, but very well on a surface of platinum covered by a slight coating of oxide, whilst that it is the contrary takes place when the acid is very much diluted.

In order that these actions should produce even small deviations of the needle of the galvanometer, it is necessary to have large surfaces of platinum; this is the reason why the deviations are so sensible with the spongy platinum; it was also for this end that I employed a large capsule of platinum. I have even tried the effect by putting to one of the extremities of the galvanometer a platinum wire immersed two or three centimeters only in the liquid, and to the other end spongy oxidized platinum: I had a current of from 5° to 6° only. Replacing the wire by a morsel of the sponge not oxidized, or by a very extensive surface of platinum, I had a current which made the needle deviate more than 90°.

It is evident that in all these phenomena it is a chemical action which is due to electricity that is there developed, and that the oxidation of the platinum has a great part in it. Still, what is this action? This is a point which appears to me to merit a further examination. It is not impossible that it consists, at least in the case of nitric acid, in a decomposition of that acid produced by the oxygen of the oxidized surface of the platinum, as that takes place

with the peroxides, and remarkably so with those of lead and manganese. I shall return to this point, when studying at the same time the circumstances that take place with the other metals, such as gold or palladium, which have analogous properties to those of platinum.

SECTION III.

Of some other Phenomena which may be attributed to the Oxidation of Platinum.

One phenomenon that proves the facility with which platinum is oxidized, is the state into which a platinum wire constitutes itself after having been employed for some time for an aphlogistic lamp. We take a platinum wire, formed into a helix, of about a demi-millimeter in diameter; it is cleansed carefully by putting it into nitric acid, and then washing it several times in distilled water; it is then dried and placed on a spirit lamp. The lamp is then lighted in such a manner that the wire becomes red hot, the flame is then extinguished, and the wire remains incandescent by the effect of the vapour of the lamp. If we continue the phenomenon during twenty-four, or still better forty-eight hours, the wire, which was perfectly bright before the experiment, presents, after it has ceased, a greyish pulverulent surface perfectly similar to the surface of wires that have been used for the alternate disengagement of oxygen and hydrogen. This effect can only be due to the alternative oxidations and reductions which the wire has undergone by the action of the oxygen of the air and the vapour of the alcohol. This is a phenomenon altogether similar to that presented by copper wires exposed to the action of a spirit lamp, which, by the alternate oxidations and reductions they are submitted to, soon become reduced to powder. I have repeated this experiment several times, and I took care to employ perfectly pure alcohol, and threads of amianthus instead of a wick of cotton; I always succeeded very well. The wires whose surfaces have thus become pulverulent, are afterwards much superior to others for the aphlogistic lamp; they become red hot on a much larger extent of surface, and with much greater facility; a new proof that the phenomenon of the aphlogistic lamp is in a great measure due to a succession of alternative oxidations and reductions which facilitate the disaggregation of the metal.

I have likewise rendered pulverulent the surface of a platinum wire well cleansed, by making use of it to inflame hydrogen in the air, instead of a piece of spongy platinum. The wire, after having been several times removed, and during from ten to fifteen minutes each time rendered incandescent by hydrogen, has taken a greyish surface, slightly pulverulent. This experiment succeeds still better by raising the temperature of the platinum helix by means of a voltaic current, which heats it without making it red. A current of hydrogen mixed with atmospheric air, by means of a double-mouthed

stop-cock, is then directed on the helix, and at the end of a certain time the surface of the platinum has taken the grey appearance already described.

Will not the preceding experiments prove that the phenomena observed by Dœbereiner are due to a succession of alternate oxidations and reductions? The surface of platinum oxidized in the air will be immediately reduced by the hydrogen, then oxidized anew, again reduced, and so on. These alternations succeed each other very rapidly: the result is an elevation of temperature that renders the platinum incandescent, and ends by inflaming the hydrogen. What seems to support this explanation is, that the circumstances that favour the greatest oxidation of platinum are those also which are exacted for the success of the experiment of Dœbereiner. Thus, for example, Dalony and Thenard have remarked that the platinum, in order to become incandescent under a current of hydrogen in the air, ought to be perfectly pure, and that it is useful, for this end, to wash it in nitric acid; the extent of surface, its state of division, also favour the production of the phenomenon. Let us carefully read all the works that have been written on this subject, and particularly those of Faraday, and we shall find that the most favourable conditions for determining intermedially by platinum the combinations of oxygen and hydrogen, are those also which are the most proper for facilitating the oxidation of this metal.

The explanation I have just given may be objected to: 1st, That there are cases where the action of certain substances determine the combination of oxygen and hydrogen, without admitting a succession of alternate oxidations and reductions; 2ndly, That platinum in its spongy state itself determines certain combinations, which cannot be accounted for in the same manner as I have just explained in the combinations of oxygen and hydrogen. We ought therefore to conclude, if these objections are well founded, that it is necessary to acknowledge in the experiments of Dœbereiner, the action of a new force which Berzelius had named *the catalytique* force.

As to the first objection, I observe that the cases in which there is a combination of oxygen and hydrogen, by the intermedial effect of certain substances for which it is impossible to admit a series of oxidations and reductions, are those in which we make use of a very high temperature, and of bodies that are non-conductors of electricity. These are fragments of glass or porcelain, which are heated very strongly over mercury, and in which we thus develope an electrical state that ought probably to bring about the combinations of the gases by the small discharges which result from it. In fact, if the experiment be made in darkness, we see faint glimmerings, analogous to those observed in agitating mercury in a capsule of porcelain heated to a high temperature. All the cases of this class cited by Dalony and Thenard in their memoir, appear to me to be explicable in this manner. Thus they have not any relation in their immediate cause with the phenomenon discovered by Dœbereiner.

The second objection is stronger. M. Kuhlmann has described

a series of chemical combinations produced through the intermediation of platinum, which seem to conform with difficulty to the explanation that I have given of the combination of hydrogen and oxygen. However, if we study with care all the facts observed by M. Kuhlmann, we may, I believe, explain them by the ordinary chemical actions, without the necessity of having recourse to a new and special force. We must not lose sight of the fact, that if platinum can become oxidized, as I think I have demonstrated, there results from it two important consequences in the phenomena wherein platinum is brought into contact with different substances. The first is this, that when platinum takes oxygen to a compound that contains it, the other element being in a nascent state, it finds itself by that more disposed to combine with a third substance which is present. The second consequence is, that the oxygen condensed on the surface of the platinum which is ready to abandon it, finds itself by the simple fact of its condensation in a state far more proper to form a combination than when it is in the gaseous form. If we follow these two consequences in their applications, the phenomena observed by M. Kuhlmann may, with only a few exceptions, be easily explained. There is only those in which the oxygen, not intervening in an apparent manner, neither in a free state, nor in a state of combination, which present some difficulties in their explanation. But, in these cases, it will be possible that the atmospheric air, and consequently the oxygen it contains, performs a part of which there can be no doubt; it is possible also that the platinum may combine with the carbon, with sulphur, or with cyanogen, in the same manner and with the same facility that it combines with oxygen. I will not dwell any longer upon this particular point, which merits being treated in a special manner, and with the help of experiment. I shall confine myself to resuming my idea, in saying that the superficial chemical actions, of which the surface of platinum is susceptible, appear sufficient to account in a satisfactory manner for the phenomena of combinations which the presence of this metal determines, and which are generally attributed to the force named *catalytique*.

I will not terminate this paragraph without speaking of a phenomenon that appears to me likewise to be attached to the oxidation of platinum and to depend upon it. I allude to the peculiar odour remarked in the oxygen gas which is disengaged at the positive pole, when we decompose water by the pile, and when we make use of this liquid by putting it in the circuit of wires of gold or platinum. There is no one who having often operated with the pile, who has not been struck with this odour; as for myself I have often observed it: but M. Schoenbein is the first who has studied it with any particular care, and who has made it the object of very interesting researches.*

M. Schoenbein has been struck with the analogy that exists be-

* *Bibl. Univ. nouvelle série*, t. xxviii, p. 342 (Août, 1840).

tween this odour and that which is left after the discharge of ordinary electrical machines, and the fall of the electrical fluid from the atmosphere. He thinks it ought to be attributed to a particular substance extremely subtle, of the same kind as chlorine or fluor, and which, like them, would have a great affinity for oxidable bodies, and which being spread in the air and in the water in a state of combination, would be separated from bodies with which it is combined by the decomposing force of electricity. He proposes to name this new substance *ozone*.

I must avow, that in repeating carefully the ingenious experiments of M. Schoenbein, to which I added some others, I arrived at a different conclusion to his. I shall not insist on the different objections which his hypothesis appeared to present to me. I shall confine myself to pointing out two of them. The first is, that I do not clearly see with what substance the ozone is found combined in the air and in the water whence electricity disengages it. Is it with hydrogen? But in that case, there ought then to be an excess of hydrogen in the decomposition of the water, and we ought to find hydrogen in the air, which also contains ozone. The second objection is, that M. Schoenbein admits that instantaneous electric discharges, such as from a Leyden jar or from a thunder cloud, have a decomposing virtue. Now, I do not believe that it is thus, and I think that in order to separate oxygen and hydrogen, and consequently ozone, a continuous electric current is necessary. I will add, in fine, that inasmuch as azone has not been isolated, it will be difficult to admit its existence, at least to be able to invoke in its favour proofs so conclusive as those that exist for the existence of fluor.

In repeating the experiments of M. Schoenbein, I have been convinced, as he was, that it is not necesary to seek for the cause of this sulphurous or phosphorous odour in strange substances mixed with the water that we decompose, such as the acids. But I am still at a stand on the idea that I had before the publication of the work of M. Schoenbein, namely, that this odour proceeds from the metal which is used at the pole of the pile in the liquid.

I at first proved, with a very strong pile, the fact of the transportation of very finely divided metallic particles from the + pole to the — pole in the air, as a vacuum, in making use as poles of two morsels of spongy platinum very near together, but not in contact, in place of two charcoal points. I have even obtained some traces of the odour in this experiment. I have remarked, that when we decompose water pretty well acidulated, with a very strong current, by means of two wires, or two plates of platinum which have not a very great surface, we see at the end of a certain time, and when the liquid traversed by the current is strongly heated, a deposit of a black powder, which is platinum divided; the platinum appears to be detached from the wire and the plate which is used at the positive pole. I am disposed to believe then, that the current which comes from the positive pole, draws with it extremely attenuated par-

ticles of the metal of which the pole is made, which thus remain in suspension in this gas, to which they give by their presence the odour in question. This odour will then be due to very finely divided molecules of a metallic oxide.*

The presence of oxidable bodies ought, as M. Schoenbein has observed, to make this odour disappear, because the immediate result from it is the oxidation of the platinum. The elevation of the temperature of the liquid will likewise produce the same effect, because it reduces the oxide. It is really a remarkable thing, that when the current is very energetic, and it has traversed the liquid long enough to heat it considerably, there is no longer any trace of the odour.

All these results, proved by experiment, agree very well with my hypothesis; but there is one fact which appears to me to be eminently favourable to it. If we make use, for the decomposition of water slightly acidulated, containing for example $\frac{1}{15}$ in volume of sulphuric acid, of a pile of constant force of ten elements, feebly charged, we have no trace of the odour, by taking for the poles two plates or two long wires of platinum. But if, without changing the plate or the wire, we diminish the surface of the wire which forms the positive pole, very much, either by covering it with wax, and thereby not leaving it in immediate contact with the liquid, only at some points, or by enclosing the platinum in a glass tube, in such a manner that its extremity alone is in contact with the liquid, we then obtain, though the current be rather feeble, since it does not pass the liquid with such facility, an excessively strong odour. Farther, we find when the decomposition has lasted some time, those parts of the platinum by which the current could escape, slightly corroded. Thus it is when the current, being restrained in its passage from the positive pole in the liquid, becomes thereby condensed on some points, and in making its escape it draws along these metallic particles, which, when oxidized, give out the odour in question. With a very strong pile the odour will be manifested in general more easily, because the current, being considerable, will meet with resistance in passing through the same conductor which offers no restraint to a current of less intensity; but when even the current is feeble it will produce the odour, provided that its transmission is restrained.

As to the odour which accompanies electric discharges, and the fall of the thunderbolt, it is still more easy to prove that it is due to very finely divided substances, which the electricity has transported with it. The fact of this transport has been proved by the numerous experiments of Priestley, who has shown that the spark taken from the conductor of a machine is charged

* These oxidized metallic particles which are detached from the platinum wire, explain how it is, as we have before seen, that the proportion of oxygen which disappears in the decomposition of water, is always greater than that of hydrogen, especially if the current is strong.

with metallic molecules emanating from the conductor, and which go in the direction of the positive electricity. Fusiniere has likewise shown the existence of this transport by the thunderbolt. The aura, which positive electricity produces when it proceeds from a conductor, is probably only due to the combustion of the extremely fine particles which this electricity bears away with it. Now, it is in these finely divided and oxidized molecules that the origin is found of the odour that accompanies these discharges, especially when they are intense, and when they have been continued for a long time. All the experiments cited by M. Schoenbein come to the support of this explanation. The fact that the water arrests the odour, proceeds from this, that the water intercepts the metallic emanations from the conductor; the influence of the heat that reduces the oxides ought also, as experiment has shown, to hinder the production of the odour. In fine, the two examples reported by M. Schoenbein, of a strong odour produced after the fall of the thunderbolt, are equally favourable to my hypothesis, since it is on steeples, and consequently on metallic conductors, that the lightning fell in both the cases in question.

One objection which may be presented against the explanation that I have just given, is the identity that exists between the odour produced by electricity, whatever be the nature of the metal it proceeds from. Now, if the cause of this odour resides in the metallic oxide, this odour ought to vary with the nature of this oxide. I remark first, that it is quite natural that bodies so similar as the oxides of platinum, of gold, of silver, and of copper, should have an analogous odour. Again, the sense of smell is not so perfect that it can distinguish the very delicate shades between these different odours; and in inspiring the gas which possesses the odorous principle, in such a manner as to appreciate the odour still better, by bringing into action at the same time the taste and smell,* I have found, and I am not the only one, sensible differences in the effect produced on the two organs by the principle in question; it following that if the wires which conducted the current into the acidulated water were of platinum, or of gold or silver, the impression was much more disagreeable and acrid with platinum and silver than with gold. With copper wires I have also obtained an effect, less, it is true, than with the other metals, because the oxide of copper is almost entirely dissolved in acidulated water, but this effect was likewise of a nature rather different from that obtained with platinum, or with gold or silver. In all these experiments, the water I made use of was distilled water, mixed with a small quantity of very pure sulphuric acid.

I will add again, that in operating with golden wires, I obtained at the commencement a strong odour; the gold wire made use of at the positive pole was then seen by degrees to take a reddish ap-

* This mode of operating was pointed out to me by M. Melly, who had it from M. Schoenbein himself, whose experiments he witnessed.

pearance, indicating that it was becoming oxidized, the oxygen gas and the odour diminishing at the same time, until the wire had taken altogether a red colour. The poles were then changed immediately; the wire, in place of remaining red, presented a pulverulent surface, due to the reduction of the oxide; and if the wire was again brought into communication with the positive pole, the odour became manifest with great force at the commencement.

I need not add, that the negative polarity of so decided a character which wires of platinum and gold acquire when plunged into oxygen charged with the odorous principle, is very probably due to the deposit of oxide in suspension, which operates on their surfaces. Now, as I have already shown, platinum in a finely developed state, when it is oxidized, is eminently negative, if it forms a voltaic pair with beaten platinum, either in wire or in plates. It is also probably to the molecules of platinum or of gold, transported by the current on the wires or plates, serving as negative poles, and where the disengagement of hydrogen completely reduces them, that the strong positive polarity which these wires and plates possess is due. This polarity is much stronger than that acquired by these metals on being simply immersed in ordinary hydrogen.

I will not conclude this first section without remarking, that the digression I have allowed myself to fall into on account of the *ozone* of M. Schoenbein, is connected with my subject, since the basis of the explanation I proposed is the faculty which platinum and gold possess of becoming oxidized. This is, then, an application of this property which appears to me to be not without some interest. I hope that M. Schoenbein will pardon me for differing from his opinion on a question in which, in every case, he will always have the merit of being the first investigator, and of having singularly advanced it by his numerous and beautiful experiments. In conclusion, I present my opinion with deference, and I shall be ready to acknowledge my error when pointed out to me.

MEETING OF THE BRITISH ASSOCIATION.

(Selections continued from page 138.)

"On some new Oxides of certain of the Metals of the Magnesian Family," by Dr. Lyon Playfair.*—Dr. Playfair first adverted to our defective knowledge of the history of the magnesian oxides. Chemists were more intimately acquainted with this family than with any other; but still the actual amount of their knowledge was very scanty. Manganese, for example, possesses six degrees of oxidation; whilst magnesium, the type of the family, possesses only one. Iron and chromium possess sesqui-oxides, but copper and zinc do not;

* We have before alluded to this paper (page 129), but could not at that time give the particulars contained in it.—Edit.

yet a complete identity in the structure of their molecules has been pointed out, affording new proofs of chemical analogy. Dr. Playfair showed a diagram representing all the magnesian oxides now known, in contradistinction to those which analogy leads us to expect. In this tabular statement, he denied the existence of peroxides of copper, zinc, and calcium, although it is well known that these oxides are admitted by chemists as having been discovered by Thenard. The compounds obtained by Thenard, and to which he had the general formula $R\ O_2$, were obtained by the action of peroxide of hydrogen on the protoxides of the metals. But these bodies did not possess any of the characters which analogy would lead us to ascribe to magnesian peroxides. In fact, they possess all the properties of peroxide of hydrogen itself: they decompose spontaneously, and detonate with combustibles. Potash accelerates their decomposition, while acids, under certain circumstances, retard it. Their action with acids is opposed to our conceptions of such unstable peroxides; for they dissolve in cold acids without decomposition, which even peroxide of manganese, stable as it is, cannot do. All these anomalous properties led Dr. Playfair to look for another composition for M. Thenard's compounds. In the course of his examination of the magnesian oxides, he found that the peroxides had a great disposition to unite with the protoxides, forming compounds of the general formula $R\ O + R_2\ O_4$. Now, hydrogen itself is a magnesian metal, or, at all events, possesses the characters of one, and its peroxide should share the disposition to unite with protoxides. The author, therefore, drew the conclusion, that Thenard's compounds were in reality similarly composed, having the formula $M\ O + H_2\ O_4$. And in confirmation of this view, he stated, that Thenard's own analyses coincide with it much better than with his own view of their being peroxides. This chemist obtained always too little oxygen to agree with his theory of their composition. He ought to have procured oxygen in the proportion of 3 : 6 : whilst on Dr. Playfair's view, the proportion would be 3 : 5, a result which approximates closely to Thenard's analyses. From these considerations, the author considered that he had a right to affirm, that the peroxides of copper, zinc, and calcium, as far as Thenard was concerned, were yet unknown, and remained to be discovered. We cannot here enter into the details of Dr. Playfair's *modus operandi*, and omit them with less regret since he stated, that his "Memoir on the true Peroxides," will shortly be published. We shall therefore confine ourselves to an enumeration and brief description of the compounds described by him. *Peroxide of copper* is of a brownish black colour. It yields oxygen on being dissolved in acids and chlorides. He obtained a hydrate of this compound oxide. By a strong heat this oxide is further decomposed. The next oxide to which the author referred, was the *peroxide of iron*. His attention had been directed, a few days since, o an ambiguous sentence in the "*Comptes Rendus*," in which M.

Fremy appears to have got *indications* of this oxide, although no description is given of it. Dr. Playfair stated that if M. Fremy had published a detailed account of it, of course the priority of publication entitled him to the priority of discovery, but he had seen no such account. The peroxide of iron resembled in character the peroxide of copper. Like that oxide it contained two atoms of water, of which it lost one and a half atom *in vacuo*. The author then described a peroxide of aluminum, which differed from the others in its relations to water. The water in this oxide played a very important part. The oxide was soluble in potash. It could be obtained in a crystallized state, and formed compounds of considerable interest, as throwing light upon the constitution of corundum and other minerals. He then announced some new oxides of zinc, the examination of which would shortly be completed. He had kept himself from examining the metallic acids which might be derived from them, as M. Fremy was engaged in their examination, and he had no wish to interfere with the researches of that able chemist. The author pointed to the necessity of doubling the atomic weights of the magnesian metals. He had shown that these peroxides possessed the formula $R_2\ O_4$, and not $R\ O_2$. Taking the case of copper we have the series

$$Cu_2\,O—Cu\,O—Cu_2\,O_3—Cu_2\,O_4.$$

In all these oxides, the copper unites in *two* atoms, except in the case of the protoxide. But by doubling the atom, we would have a uniform series

$$CuO—CuO_2—CuO_3—CuO_4$$

—the oxides in this case increasing in arithmetical progression. He showed, that in many of the salts, there was evidence of this double atom. Sulphate of copper absorbed two and a half atom of ammonia; a sulphate of lime was known with half an atom of water, &c.

"On some peculiar instances of (so-called) Catalytic Action," by Mr. Mercer.—Mr. Mercer had long considered that instances of catalysis were merely examples of chemical affinity, exercised under peculiar circumstances. A body never entirely yields up its chemical characters on uniting with other bodies. The iron in protoxide of iron, has still an affinity for more oxygen, and has not lost that affinity by its first union with that element. The intensity of affinity, by which the simple elements are joined in the complex molecule, must be the measure of the stability of the compound. Mr. Mercer argued, that when the elements of a body are in a mere static equilibrium, by virtue of a feeble attraction, and when it is acted upon by another body possessing an affinity for one of its constituents, which constituent, on the other hand, from peculiar circumstances, is not prone to combine with it, that, in such a case, so-called catalysis must ensue. Thus, on mixing oxalic acid and nitric acid with a little water, and raising the temperature to 130°, no action ensues. But if a small portion of any protosalt of manganese be now added, the decomposition immediately commences, and all the nitric

acid is converted into nitrous acid, whilst the oxalic acid passes into carbonic acid. He thus accounts for this singular action:—The carbonic oxide of the oxalic acid possesses a disposition to unite with oxygen; to gratify this disposition it endeavours to withdraw it from nitric acid, but it is not sufficiently powerful to do so; still it places the atoms of the nitric acid in a state of tension. Another body (protoxide of manganese) now being introduced, which also possesses an affinity for oxygen, exerts this affinity, and the combined forces thus acting upon the nitric acid, occasions its decomposition. The moment the oxygen is withdrawn from its state of combination, it has two affinities to choose between, and the attraction of the oxalic acid being greater, it passes over to it, converting it into carbonic acid. The protoxide of manganese still remaining, will act on fresh portions *ad infinitum*. Most of the vegetable acids may be decomposed in a similar manner. Following up this view, Mr. Mercer had discovered a number of examples of what formerly would have been called catalysis. He showed that when alumina (precipitated from a hot solution) is placed in contact with dilute nitric acid, no apparent action ensues. But as Dr. Playfair had described a peroxide of aluminum, it ought to have a disposition to unite with oxygen. To discover, then, whether the atoms of nitric acid were actually in a state of tension, he introduced a slip of calico rendered blue by indigo. When this came in contact with the precipitated alumina, the indigo was immediately discharged, although it remained unaffected in the supernatant liquor. Chlorous acid was a body well fitted for his purpose, as its elements were held together by a feeble affinity, and as its oxygen was very readily yielded. He showed that the peroxide of copper, discovered by Dr. Playfair, occasioned a great evolution of oxygen from a solution of chloride of soda. This was owing to its endeavours to become cupric acid, which, under certain circumstances, it did form. He had noticed many years since, that a dark purple solution is obtained on mixing chloride of lime, a salt of copper, and lime with water, and leaving the mixture at repose. No evolution of oxygen is occasioned by this purple solution, but by the peroxide of copper before passing into it. Peroxides of manganese and cobalt exert a similar action. The reason was, that these metals possessed only a feeble affinity to pass into the metallic acids. Still the affinity was sufficient to occasion the withdrawal of oxygen from chlorous acid. The moment it was withdrawn, elasticity came into play, and it escaped as a gas. A similar action is exerted by the peroxides of iron and lead. From these and several other instances which were described, Mr. Mercer concluded that almost all instances of catalytic action may be reduced to feeble chemical affinity. He concluded by some speculations on the atomic constitution of complex molecules.

Prof. Poweli gave an account of a simplified apparatus, for applying circular polarization to chemical inquiries. The application of the phenomena of circular polarization in characterizing certnia

liquid solutions, was fully pointed out by Biot, who also contrived a very accurate apparatus for examining the effects in question. But that apparatus being expensive and difficult of adjustment, Prof. Powell has devised a simpler instrument, sufficiently accurate for general indications, and for the objects of the chemical student He places the solution to be examined in a common test tube, which must, of course, be fixed in a vertical position; below the tube is the polarizing plate, making with it an angle of $35\frac{1}{2}°$, and above it is a double refracting crystal (rhomboidal calcareous spar) in its natural state. The light is thrown on the polarizing plate by a plane mirror; passes along the tube (at either end of which it is limited to a small aperture); is then separated by the crystal, and finally viewed through a lens which magnifies the separation, and also reduces the irregular light to perfectly circular discs. Tubes of various lengths may be used as occasion requires.

Dr. Dalton brought before the meeting three papers, lately published by him. 1. "On the Phosphates and Arseniates." 2. "On Microcosmic Salt." 3. "On a new and easy method of Analyzing Sugar." These papers having been published, it is, therefore, unnecessary to furnish abstracts of them.

"On the Agricultural Importance of ascertaining the Minute Portions of Matter derived from Organic Sources that may be preserved in the Surface Soil; and on the Chemical Means by which its presence may be detected," by Dr. Daubeny. The researches of Sprengel and Liebig, by showing the manner in which minute quantities of certain ingredients may impart to the soil into which they enter as constituents, entirely new properties with reference to the purposes of agriculture, have given additional interest to the methods of analysis which aim at determining the chemical composition of the surface, and of the substratum from which the former principally derives its chief ingredients. The rude mechanical method adopted, even by such chemists as Sir H. Davy, is no longer considered sufficient. The nature, as well as the amount of the organic matter present, and the existence of phosphates, &c., in the proportion of $\frac{1}{1000}$, or even $\frac{1}{10000}$ part of the entire mass, are points deserving investigation, and afford a clue to the description of manures most likely to be useful, and to the general treatment which the land may require. It is also obvious, that the same importance attaches to a knowledge of the constitution of the sub-soil, since the advantages of exposing to atmospheric influences, and thus disintegrating the portions underneath by deep ploughing, and other methods of bringing the subsoil to the surface, will in a great degree depend upon its containing ingredients which the crop requires for its subsistence, and of which the superficial soil has been already in a great degree exhausted. Thus, for example, it will often become a question with the farmer, whether it will be more economical to mix with the soil a given quantity of phosphate of lime, or to incur the labour of so breaking up a portion of the subjacent rock, as to un-

lock, as it were, for the use of the crop, that quantity which it contains in close union with its other constituents. This inquiry, however, presupposes a knowledge on his part of the existence of phosphate of lime in the soil, and of the relative proportion it bears to the other ingredients: data, which can only be obtained through the assistance of refined chemical analysis. A few simple and easy calculations may show how very small a proportion of this ingredient might suffice during a long period of time, for the demands even of those crops which require the largest amount of it for their nutrition. Suppose the sub-soil of a single acre of ground, turned up to the depth of a foot, to weigh 1,000 tons: now if this rock should be found to contain only $\frac{1}{1000}$th part of phosphate of lime, it will follow that no less than a ton of this substance might be extracted from the uppermost foot of the subjacent rock, by the action of the elements, or by chemical means. Now one ton of phosphate of lime would be adequate to supply 125 tons of wheat, or 680 tons of turnips. And if we reckon the average crop obtained from an acre of land to be, of wheat one ton, and of turnips, fifteen, it is evident that we have at hand as much phosphate of lime as would be neccessary for 125 crops of the former, or for 45 crops of the latter. Dr. Daubeny said he had great reason to believe, that many of our secondary rocks, those especially which contain organic remains, and which appear in a great measure to be made up of shells, would be found, if examined, to contain as large a quantity of phosphate of lime as that mentioned. Though the soil of Great Britain be found deficient in the phosphates, there is reason to believe the subsoil might, in many cases, be made, by proper management, to impart to it what was wanting. It is now some years since the discovery, by Mr. Buckland, in the lias, and other secondary rocks, of the solid fæces of certain extinct animals, consisting of phosphate of lime, induced Dr. Daubeny to test a variety of specimens of limestone, with a view of ascertaining whether traces might be found in them of the same ingredient. The result was, that phosphate of lime in minute quantities was much too commonly distributed to be attributed to cuprolitic matter, or to afford any independent evidence of its presence. When, indeed, we recollect that the shells of invertebral animals contain from three to six per cent. of phosphate of lime, and that according to Mr. Connell, the scales of extinct fish, taken from rocks as old as the coal formation, possess no less than fifty per cent. of the same ingredient, it would be wonderful, indeed, if all traces of this substance had dissappeared from rocks, which appear often to be made up in a great degree of the debris of shells and other marine exuviæ. Dr. Daubeny was, therefore, not surprised at being informed, by M. Schweitzer, who is entrusted with the management of the German Spa at Brighton, that he had detected in the chalk of Brighton Downs, as much as $\frac{1}{1000}$ part of phosphate of lime. From experiments since made by Dr. Daubeny in the same rock, taken from various localities, he was inclined to

believe, that minute portions of this substance are present, not uncommonly, in that formation. The frequent occurrence of phosphate of lime in calcareous rocks, and the probability of its having been derived from the shells, or bony matter of the living beings, contained in the calcareous rock, led Dr. Daubeny to suspect that traces also of the organic matter which contributed to make up the animal structure, might likewise be found accompanying it. To determine this, the doctor had applied a test to about fifty different specimens of limestone selected from his cabinet, and found, that whilst the solutions of the pure marble, such as that of Carrara, continue unaffected, the equally pure and white limestones taken from the chalk and tertiary formations in general, become distinctly darkened by the addition of nitrate of silver.—Dr. Daubeny read a letter from M. Schweitzer, who had been precluded from employing the secondary limestones in obtaining carbonic acid wherewith to impregnate his mineral waters, owing to an empyreumatical odour which the gas carried up, and which he attributed to an organic cause. To obtain a perfectly pure carbonic acid, for his imitation of the Spas of the continent, he was compelled to resort to the pure kinds of marbles. With regard to the presence of organic matter in the subsoil, its detection may be a matter of some agricultural interest, when we remember that the small quantities of nitrogen which are required for the growth of those vegetables that first start up in a new country could not have taken place from an accumulation of mould, by the decay of antecedent plants, but must have been derived in a great measure from the animal matter which is contained in the rock upon which they grew, and which proceeds from the exuviæ of races of beings belonging to a former period of creation. In a more advanced period of vegetation, this same material may be of some value to the crops that occupy the soil. Dr. Daubeny suggested whether the more compact texture of certain calcareous rocks than of others, might not be connected with the existence in them of organic matter, which, by its interposition, may prevent a crystalline arrangement of its particles from taking place. It may be that the attraction between the particles of matter, which, if uncontrolled, would prove too powerful for the agents of decomposition to overcome, may be weakened by the presence of organic matter, and thus be enabled to supply the vegetables that take root in it with the solid matter which their structure requires. To the geologist, too, it cannot but be of interest to trace the several steps by which the organic matter, which primarily must have constituted so large a portion of the bulk of the various extinct animals and vegetables, have disappeared from the strata which enveloped them.

Mr. Webb Hall wished, as a question of practical interest, to know whether the Doctor's discoveries would influence the mode in which calcareous matter was applied to the soil, so as to increase the fertilizing effect ?—Dr. Daubeny said, he doubted whether, in ordinary cases, the small quantity of organic matter which limestone contains

could be of great importance. Still, it was useful to know from what the first vegetables derived the nitrogen they required.

"On the Electric Origin of the Heat of Combustion," by J. P. Joule.—The author is of opinion that he has succeeded in rendering evident the fact that the heat of combustion is an electrical phenomenon, and that the method of its developement is by resistance to electric conduction. He has entered upon other cases of chemical heat, but finds them more difficult than he expected, and intends to examine the heat of combustion again, being satisfied that, when its electrical character is completely established, the theory of all chemical heat will find its proof at the same time.

Dr. Daubeny read a paper "On the Causes of the Irregularities of Surface which are observable in certain parts of the Magnesian Limestone Formations of this Country."—The magnesian limestone rock in some of the quarries in Derbyshire, presents a remarkable appearance. They do not possess an undulating surface, as the limestones generally do, but the surface is covered with irregular elevations and depressions of a very marked character. Prof. Sedgwick had cursorily noticed the configurations which these magnesian limestones possess, and ascribed it to an arrangement of the particles of the rock which took place in the act of its consolidation. Dr. Daubeny, however, was inclined to ascribe them to the action of atmospheric influences, and to that of water impregnated with carbonic acid.

"On the Composition of the Blood and Bones of Domestic Animals," by Prof. Nasse, of Marburg.—The author of this paper had made an elaborate series of proximate analyses of the blood of man, the dog, cat, horse, ox, calf, goat, sheep, pig, goose, and fowl, ascertaining the relative proportions in each of water, serum, albumen, fibrine, and fat, together also with the usual saline constituents. The results of his analyses he exhibited in a series of tables, which are too extended for insertion. He drew some conclusions with respect to the relation of each constituent part to the rest. Thus, from purely chemical evidence, he arrives at the conclusion, that the less iron and more alkaline carbonates and fibrine which are contained in the blood, the weaker will the constitution of the animal be, and the more liable to disease. Thus the blood of the English horse contains much more iron and less alkaline matter or fibre than the German horse; and it is well known that the blood is far less liable to disease in the former than in the latter. Prof. Nasse then examines the variation in the composition of wounded and of healthy bones. The conclusion deduced from his analysis is, that the bones of injured limbs are deficient in organic constituents, such as in their gelatine, as well as in the carbonate of lime. The proportion of phosphate of lime remains unchanged, but that of carbonate of lime is much diminished. The Professor accounted for this phenomenon from the circumstance of carbonate of lime being soluble in carbonic acid. When a limb is injured, blood thickens

in the substance of the bone, and in this state contains more carbonic acid than that which freely circulates, and would, therefore, favour its solution. The liquid taken from the injured part contains more than its normal quantity of albumen, which, under certain circumstances, favours the solubility of carbonate of lime.—The thanks of the Section were given to Prof. Nasse for his communication.

Dr. Playfair drew attention to some points connected with Professor Nasse's tables of the composition of the blood of the various animals. He (Dr. Playfair) had shown, by some analyses published in "Liebig's Physiology," that the ultimate composition of blood and flesh were nearly identical. Hence the professor's results might be considered as tables of the economic value of the flesh of animals, and the results agreed closely with actual facts. Thus, according to the author of the paper, the blood of man contained 74.194 of albumen; the blood of the ox, which formed, after being transformed into flesh, the most nutritious food for man, contained 74.45 of the same body; the pig, the flesh of which was equally nutritious, contained also exactly the same as that of man—viz. 74.80. Then in those animals, the flesh of which is less nutritious, we find the proportion of albumen considerably less than in man; thus in the blood of the goat it is only 62.905; in the goose 48,695; in the fowl 48.52.

"On the Improvement of the Telescope," by Mr. Fox Talbot.—Mr. Fox Talbot said, that this subject occurred to him about two years ago, when the Earl of Ross (then Lord Oxmantoun) was making much larger specula for reflecting telescopes than had ever been obtained before; and he thought, if once we had a very large and perfect speculum, it might be possible to multiply copies of it by galvanic means. He had observed, that if an electrotype cast were taken from a perfectly polished surface, the cast was also perfectly polished; so that no defect of form from this cause could have an injurious effect on the speculum. The great and obvious defect was, that electrotypes were in copper, which reflected but little light. He mentioned these ideas to Professor Wheatstone, who said the same had occurred to him; and he showed him a paper which he had drawn up some few months before, and in which he suggested the taking galvanoplastic casts of specula in platina, palladium, silver, or nickel, and for especial purposes gilding the copper; taking care that the two precipitations adhered well to each other. So that (said Mr. Talbot) the idea had suggested itself independently to both of them; but, on comparing notes, they found differences. Though it had occurred to him (Mr. Talbot) to precipitate white metals, yet he did not think that platina would have a sufficiently beautiful white metallic polish. Professor Wheatstone, had, however, made choice of platina; and varying the quantity till he found the required proportion, he obtained a mirror in platina, which appeared to him (Mr. Talbot) to have quite brilliant polish enough, and to be white enough to answer the purpose; and he considered, therefore, that

Professor Wheatstone had proved, that, at least in one form, the specula of telescopes might be made by voltaic precipitation. His own idea was, that it might be possible to whiten the surface of the copper without injuring the form; and, therefore, having obtained a speculum in very bright polished copper, he (Mr. Talbot) whitened it, and transformed it into sulphuret of copper; and, after having retained it about a year, he did not perceive the smallest alteration in any respect. This, therefore, appeared to him a mode by which important results for astronomers could be obtained. For the last year, perhaps, nothing further had been done, either by Professor Wheatstone or himself; but, the other day, being at Munich, he (Mr. Talbot) visited Professor Steinheil, who showed him his inventions, and told him he had discovered a method of making specula by the electrotype. It so happened, that both Professor Steinheil and himself had published their respective methods about a month or six weeks before; the professor having read a communication on the subject before the Academy of Sciences at Munich, and printed it, and he (Mr. Talbot) having published his in England. Their modes were, however, different, as Professor Steinheil precipitated gold upon the speculum of copper: and, having precipitated a certain thickness of gold, he then precipitated copper on the back of the gold, to give it sufficient thickness. He (Mr. Talbot) should have thought beforehand that gold would not reflect light enough to be available; but Professor Steinheil informed him he had found, by careful experiment, that it reflected more light than polished steel. He allowed Mr. Talbot to look through a Gregorian reflecting telescope, of which the speculum was a common one, but gilded, and he found the image perfectly clear and well defined. A slight tinge of yellow was thrown over all the objects, but the image was perfectly clear and defined. Professor Steinheil said, that in the course of a year, he should have a very large telescope, furnished not only with a speculum, but also with other apparatus, voltaically formed, so that telescopes might be made all from a good model, so as to insure greater accuracy of proportions; and in this way even very large telescopes might be constructed at a comparatively trifling expense. With reference to precipitating copper on the back of the gold, the professor had a simple expedient for securing adhesion. He first precipitated gold from the cyanide of gold, and he mixed with it cyanide of copper, and kept gradually increasing the quantity of the latter sort; so that an alloy was precipitated, which was continually increasing the copper with respect to the gold, till he had a speculum whose surface was gold, and which then became an alloy, the quality decreasing, till, at the bottom, it became pure copper. This was important: because, without such experiments, one would not have known that such results would have followed; for some philosophers supposed, that, if we attempt to precipitate the salts of two metals, only one was precipitated; but Professor Steinheil informed him that they

precipitated in union. He thus obtained a speculum with a face of gold and a back of copper. But, supposing the largest, cheapest, and best speculum were obtained, the framework of the telescope would be so gigantic, that few observers would be able to use the instrument. With a focal length of sixty to eighty feet, it would be quite unmanageable for any private individual. The idea occurred to him (Mr. Talbot), to have a tube fixed in an invariable position, and to have a perfectly true plane mirror, of a size somewhat larger than the concave speculum, placed in front of the tube, with an aperture in the centre. This plane reflector should be moveable about its centre in any direction; so that luminous bodies, falling first upon the plane reflector, were then reflected against the concave reflector, and passed through the aperture. The only motion requisite for its plane mirror would be one about its centre. The mechanical difficulties in the way of this plan would be far less than in the common method. Professor Steinheil's idea on this point was somewhat different. He (Mr. Talbot) did not think it important in what direction the tube of the telescope was directed. Professor Steinheil's idea was, that it should be pointed directly to the pole of the heavens, and kept as steady as possible, and that the plane mirror should have a simple motion of revolution, indeed two motions, but about a rectangular centre.

Shortly after this paper had commenced, the distinguished astronomer, Professor Bessel, entered the Section. As soon as the President announced his name, which he accompanied with a few eloquently eulogistic sentences, the entire Section rose from their seats and applauded. M. Bessel expressed in warm terms his sense of the honour.—Mr. Isaac Holden observed that the late Earl Stanhope had actually constructed a reflecting telescope on the very principles now proposed, both with respect to the fixity of the concave speculum, and the use of a moveable plane mirror.—Sir D. Brewster mentioned a plan proposed by an American some years since, for generating a pro-tempore speculum, by causing quicksilver to revolve rapidly, when the centrifugal force would form it into a paraboloid, the very shape best adapted for the purpose.—The President inquired from Mr. Talbot whether some contrivance similar to the aërial telescope of Huygens and Hevelius might not be adapted so as to dispense with the plane mirror, the accurate construction of which was nearly, if not quite, as difficult a mechanical problem as the construction of the great speculum.—Mr. Talbot replied, that the principle of the aërial telescope was not, in his opinion, applicable to a reflector. The reflecting speculum being on the ground, it would be necessary, on that principle, that the observer should be elevated: an arrangement incompatible with his free change of place. In reply to a question from Sir T. Brisbane, Mr. Talbot said, that, with proper precautions, the original speculum would not run any risk of deterioration during the electrotype process.

Sir D. Brewster then proceeded to make three communications: —1, "On Luminous Lines in certain flames corresponding to the defective lines in the Sun's light:" 2, "On the Structure of a part of the Solar Spectrum, hitherto unexamined;" and 3, "On the Luminous Bands in the Spectra of various flames." 1. After noticing Fraunhofer's beautiful discovery as to the phenomena of the line D in the prismatic spectrum, Sir David said, he had received from the establishment of that eminent man, at Munich, a splendid prism, made for the British Association, and one of the largest, perhaps, ever made; and, upon examining by it the spectrum of deflagrating nitre, he was surprised to find the red ray, discovered by Mr. Fox Talbot, accompanied by several other rays, and that this extreme red ray occupied the exact place of the line A in Fraunhofer's spectrum, and equally surprised to see a luminous line corresponding with the line B of Fraunhofer. In fact, all the black lines of Fraunhofer were depicted in the spectrum in brilliant red light. The lines A and B turned out in the spectrum of deflagrating nitre to be both double lines; and, upon examining a solar spectrum under favourable circumstances, he found bands corresponding to these double lines. He had looked with great anxiety to see if there was any thing analogous in other flames, and it would appear that this was a property which belonged to almost every flame. II. He had, by means of the prism from Munich, been enabled to extend the solar spectrum beyond the point where, according to Fraunhofer, it terminated immediately at the side of the line A, and he (Sir David) found one part to consist of about sixteen lines, placed so near to each other that it was difficult to recognise the separation; but the lines, as they approached to A, were much nearer to each other than as they receded from it; consequently, that portion of the spectrum appeared concave, resembling so much the scooped-out lines of a moulding on wood, that it was scarcely possible to suppose that the beholder was not looking at such a moulding. He was led to observe an analogous structure near the line B; and upon carrying on this comparison of structure of one part of the spectrum with that of another, it seemed to him, that, by and bye, something important would result; for there was a repetition of a group of lines, and similar lines, through different parts of the spectrum, as if the same cause which produced them in one part, produced them in another. III. He had endeavoured to procure all the minerals and artificial salts and other substances capable of combustion which could be had; and, in order to have a suitable combination, he used an oxygen light analogous to the Bude light. Every one conducting these experiments was aware that it was necessary to pass the light through a narrow aperture; but this would reduce the intensity of the light so much, as to make it difficult to observe the rays at the extremity of the spectrum; but he found that he could obtain the effect of a small aperture, by merely inclining the prism; so that, with a good rism, the great lines in the solar spectrum might be seen by using

an aperture three or four feet wide, the whole breadth of the window, by the mere inclination of the prism, which had the effect of producing a narrowing, facing the light. He had obtained 200 or 300 results, which he had not had any leisure to group; but he would mention some of the general results. When nitrate of lead was thrown into combustion, remarkably fine lines were produced in the spectrum. The luminous line D, of Fraunhofer, existed in almost every substance, especially in all into which soda entered, particularly in the flame of a common tallow candle; probably owing to the muriate of soda existing in the tallow. The hydrate of strontytes gave the lines very remarkably in yellow and green. The iodide of mercury did the same. Also in that remarkable substance, the lithoxanthemate of ammonia, first discovered and published by Mr. Fox Talbot, the fine lines were seen throughout the whole length of the spectrum; and there was a remarkable blue band, which he (Sir D. Brewster) had not distinctly recognized in any other flame. Indigo gave fine green and orange lines at equal distances from the D of Fraunhofer. Prussian blue did the same; calomel, nitrate of magnesia, litharge, also showed lines; the sulphocyanite of potash gave a violet and orange flame, with the lines extremely distinct. He hoped, at the next year's meeting of the association, to be able to embody these various results in a regular report.

Sir W. Hamilton said it was clear that these optical researches gave definite characters to things, which it would be necessary to include in any new history of chemical compounds.—Mr. Fox Talbot expressed his gratification, that Sir David Brewster had taken up this curious branch of inquiry. Nothing was more extraordinary than the first fact discovered by Fraunhofer, that the double yellow ray produced in most flames, especially when soda was in combustion, should answer exactly to the double black line in the solar spectrum. This was one of the most unintelligible things in natural philosophy.—Sir D. Brewster said that he might mention that the lines D were wanting in all spectra of the stars hitherto discovered. Fraunhofer, in his paper on the subject, stated that he had found, in all the spectra of the stars he had examined, black lines, but not the bright line D. He (Sir D. Brewster), in making experiments on the light of some of the coloured stars, particularly the blue and red stars, in many parts of the heavens, looking at them through a rock-salt prism at an angle of 79°, the largest angle that would transmit the light, and with Sir James South's telescope, he found those black bands existing in the star, and that those coloured rays were wanting which would account for the peculiar colour of the star; so that the peculiar colour of red, orange, or green stars was to be explained by the want of those rays necessary to make white light. Sir David mentioned one star in particular (we think *a* Herculis), as exhibiting this peculiarity.

Mr. Eaton Hodgkinson made a communication, "On the mode

of conducting Experiments on the Resistance of Air."—Mr. Hodgkinson said that, having been honoured by the Association with a request to pursue some experiments on the resistance of the air, he was desirous of exhibiting an instrument prepared for making the first series of these experiments. He proposed, in the first instance, to seek for the force of the wind moving at different velocities, upon plane surfaces of given dimensions, these surfaces being either perpendicular, or inclined at any angle, to its current; to determine this, he intended to place the apparatus upon the front of the first carriage of a railway train; the road along which the train passed having for a short distance poles stuck up, 100 or 200 yards asunder. He would try the experiment only on days when there was no perceptible wind; and then, if the time in seconds taken in passing between two poles be carefully observed, and the pressure indicated upon the discs (which were of two and of four feet area, both round and square), the resistance per square foot, with a given velocity, would be obtained. He hoped to determine these facts, with various velocities and at different angles of inclination in the discs, trying the same experiments with both discs at the same time, to ascertain whether the resistance to a square surface and a round one of equal area, was the same, and that the results might correct each other. The directors of the Manchester and Birmingham Railway had kindly consented, at Mr. Buck's request, to allow him to make these experiments; and he was indebted to Mr. Fairbairn for the apparatus. This was placed on the table. It consists of two discs of wood (which may be of any form), made inclinable at any angle, by means of screws, and having an attached quadrant to measure the angle. To ascertain the force of the wind, one of Salter's balance springs is placed behind each disc, attached to the cross piece which connects the two rods of the discs; and this indicated the force of the wind at any moment.

Professor Stevelly inquired whether a registering pencil was proposed, as, he conceived, if not, that the index would be in too constant motion to be observed with any accuracy. He also remarked, that a conversation had taken place in the section at Plymouth on this subject, and Mr. Phillips proposed to avoid the necessity of waiting for calm days, by observing both when the train was going and on its return, and that thus, the effect of the air's own motion would disappear.—Mr. Hodgkinson did not think a registering pencil would be required; but if, during the progress of the experiments, he found that it was so, he would adopt it. He wished to avoid, if he could, introducing the element of the motion of the air itself at all, as, from the irregularity of those currents, he was not sure a perfect elimination of them could be obtained.

Mr. Hodgkinson then read a notice of his "Experimental Inquiries on the strength of stones and other materials."—After noticing the present state of knowledge (we might say of comparative ignorance) on this subject, and the experiments of Barlow,

Rennie, and of experimentalists on the continent, Mr. Hodgkinson said, he had long felt anxious to ascertain how the three forces, the crushing, the tensile, and the transverse strength, and the position of the neutral line (that separating the extended and compressed fibres in a bent body), were connected in bodies generally: and his experiments had for several years been directed to discovering facts upon each of these matters, in order to determine the question. His experiments some years ago, made for the British Association, with respect to the values of hot and cold blast iron, had shown that the ratio of the forces of ultimate tension and compression was nearly constant in all the species of cast iron; and a few experiments made at that time on sandstone and marble, had led him to suspect that nearly the same would be the case in these and other hard bodies. Through the liberality of his friend Mr. Fairbairn (who had, as usual, given him every assistance his establishment afforded), he (Mr. Hodgkinson) had made a great many experiments upon wood, sandstones, marbles, glass, slate, ivory, bone, &c., to ascertain the tensile, crushing, and transverse strength of each: also, as far as possible, the situation of the neutral line. He had sought for these in thirteen kinds of timber, including oaks, pines, teak, &c.; all the different sorts of experiments were made, as far as possible, out of the same specimen in each case. The wood was of good quality, and perfectly dry, having been chosen for this purpose, and laid in a warm dry place for four years or more. After describing the mode and character of his experiments on the various substances named above (specimens of which he produced), Mr. Hodgkinson gave the following summary of their comparative results on marble stones of various degrees of hardness:—

Description of Stone.	Crushing force per square inch, called 1,000.	Tensile force per sqr. inch.	Transverse strength of bar 1 inch square, and 1 foot long.
Black marble	1,000	143	10.1
Italian marble	1,000	84	10.6
Rochdale flagstone	1,000	104	9.9
High Moor stone	1,000	100	..
Stone called Yorkshire flag	1,000	..	9.5
Stone from Little Hulton, near Bolton	1,000	70	8.8
Mean rates	1,000	100	9.8

or calling the mean crushing strength per square inch, in the different articles experimented upon, 1,000, we have,—

Crushing strength, 1,000.	Tensile strength.	Transverse strength.	Ratio of mean tensible to crushing strength.
In timber 1,000	1,900	85.1	1 to 0.55
Cast iron 1,000	158	19.8	1 „ 6.6
Glass (plate and crown) 1,000	123	10.	1 „ 7.8
Stone and marble 1,000	100	9.8	1 „10.5, or, 8.9, taking the hardest only.

The ratio of the crushing force to the transverse force is nearly the same in glass, stone, and marble, including the hardest and the softest kinds. Hence, if we know the transverse strength, in any of these bodies, we may predict the other; and, as glass and the hardest stones resist crushing with from seven to nine times the energy that they do being torn asunder, we may get an approximate value of the tensile force from the crushing force, or *vice versa*. These results render it probable, that the hardest bodies, whether cast-iron, glass, stone, or marble, admit of certain atomic displacements, either in tearing asunder or crushing; these displacements being in a given ratio to each other, or nearly so. In future calculations as to the strength of bodies, the crushing strength ought to be made the fundamental datum, for the reasons shown in this notice. The ratio of the transverse strength to the crushing strength is greater in cast-iron than in glass, marble, and sandstones, arising from the ductility of that metal. The necessity of enlarged inquiries in these matters will be seen, when it is reflected that calculations of the tensile strength of cast-iron, or marble, or stones in general, made from the transverse strength by the modes used by Tredgold, Navier, and others, give the tensile strength twice or three times as great as it ought to be.

The President observed, that Mr. Hodgkinson was too well known to the members of the Section to make it requisite to point out his peculiar accuracy and success as an experimental investigator; the fact, however, that he had been last year awarded the Royal Medal showed the value which the Royal Society attached to his researches. The field upon which he had now ventured was of the utmost importance, even in a national point of view; since, without a knowledge of the strength of materials, we could not hope to raise durable structures without waste: we could not unite stability and economy.—Mr. Hodgkinson thanked the President for the kind terms in which he had adverted to his feeble exertions in the cause of practical science. The building in which they were assembled was, indeed, an exemplification of the importance of the researches which he had been bringing under their notice; as he had proved to the Committee of the House of Commons, that the cast-iron pillars on which it stood were, by one-half at least, too strong for the weight they were called on to support.

Mr. John Davies, of Manchester, then read a paper "On the Manufacture and Purification of Coal Gas."—Besides the illuminating gases obtained by the distillation of coal, other gases are at the same time evolved, which are not adapted for the intended purpose. These gases are carbonic acid and sulphuretted hydrogen. The latter is particularly objectionable both from its offensive odour, and from other noxious properties. A volatile hydrocarbon usually accompanies the coal gas, and adds materially to the illuminating powers. It is well known that its two former constituents are removed from coal gas, by means of lime; but if the purification be carried too far, the hydrocarbon is also removed. Dr. Ure had shown this in the case of olefiant gas, and Mr. Davies was able to testify to the accuracy of his experiments, and to extend the remark to other hydrocarbons, which are occasionally evolved. The best means of avoiding this loss of illuminating properties is, to employ a coal containing a smaller proportion of sulphur than usual. Mr. Davies then adverted to Mr. Phillips's patent for removing ammonia from coal gas, by passing it through a purifier containing a solution of alum. He had found it, on several occasions, perfectly successful. He then adverted to the origin of the ammonia obtained in the distillation of coal. He did not think that the quantity of nitrogen contained in coal could sufficiently account for its formation. The analyses of Regnault and Richardson have, however, shown that nitrogen is contained in notable quantity in all kinds of coal.

In the course of a conversation which followed this paper, Mr. Leigh, of Manchester Gas Works, stated, that as much as two ounces of muriate of ammonia exist in one gallon of gas water.—Mr. West had examined many specimens of coal, and had never met one specimen free from sulphur. This sulphur was not always in combination with iron.—Mr. Blyth mentioned the curious fact, that in the water of a coal mine, which he had lately examined, a large amount of silicate of soda existed.

Dr. Schunk then read the paper "On the Formation of Cyanuret of Potassium in a Blast Furnace," by Dr. C. Bromeis, of Cassell.—M. Zincken discovered at the bottom of the blast furnace at Mägdesprung, in the Hartz Mountains, a mass which Dr. Bromeis found to contain ferro-cyanuret of potassium. The furnace, from which it was obtained, had been fed with *charcoal.* The other ingredients of the saline mass were, caustic potash, carbonate, silicate, and manganate of potash, together with a large portion of cyanate of potash, and cyanuret of potassium. It is probable that the ferro-cyanuret of potassium did not exist ready-formed in the mass, but was produced after dissolving the cyanuret of potassium in water. The cyanite, of potash, by its decomposition, gives rise to carbonate of potash and ammonia. Dr. Bromeis supposes that the formation of the cyanogen must have been occasioned in the following manner:—the nitrogen of the atmosphere, being exposed to a great pressure and high temperature, combined directly with the carbon of the

carburet of potassium, producing thereby cyanogen and cyanuret of potassium. This explanation accords with the experiments of Defosse.

Mr. Leigh read a communication "On a new product obtained from Coal Naptha."—The substance described was obtained in the course of some investigations on an oil which Mr. Leigh discovered about three years and a half ago, as the result of a mixture of nitric and sulphuric acids on purified coal naptha. In their behaviour with potassa, both in aqueous and alcoholic solution, the crystals now brought under the notice of the section by Mr. Leigh have much analogy with the oil (like that of bitter almonds) obtained at the same time with them. The oil when extensively exposed to the action of hydrogen, becomes a crystalline solid, having much the same appearance as these crystals. It is probable the crystals differ from the oil in containing a quantity of oxygen. Mr. Leigh had made no analyses of these compounds.

Mr. Croft read the next paper, "On Kakodylic Acid, and the Sulphurets of Kakodyl," by Professor Bunsen, of Marburg.—In the present paper, Professor Bunsen examines the higher stages of oxidation of kakodyl, and the sulphurets corresponding to them. He finds that, by the oxidation of alkarsin, either by the direct action of the air, or by means of oxide of mercury, kakodylic acid is formed; but there is also an intermediate oxide, which cannot be obtained in a state of purity, which seems to be similar to the hyponitric acid, and to be a combination of kakodylic acid with the oxide. Kakodylic acid crystallises out of alcohol; its composition is $C_4\ H_6\ As_2\ O_3 + HO$, this atom of water being constitutional, and only to be replaced by a base; it is soluble in water, but not in ether. A very remarkable fact with respect to this body is, that the poisonous properties of the arsenic seem totally annihilated; eight grains administered to a rabbit exerted no poisonous action. Kakodyl combines directly with sulphur, forming the proto-sulphuret which has been already described. This compound takes up another atom of sulphur, and produces the bisulphuret. There appears also to be a tersulphuret analogous to kakodylic acid; Professor Bunsen has not, however, been able to obtain it in a pure state. From the above results, it appears, that kakodyl is precisely similar in its behaviour to some simple metals, and the formation of kakodylic acid by direct oxidation, is in exact opposition to Dumas's theory of substitution.

Dr. Schunk read a paper "On the compounds of Carbon and Iron," by Dr. C. Bromeis.—Dr. Bromeis analyzed various kinds of iron by burning them in a tube, with a mixture of chromate of lead and chlorate of potash. The combustion is conducted exactly like an organic analysis, and is the method invented by Regnault. An important point in the determination of the carbon in iron, is to ascertain the proportion of carbon in a state of combination, in contradistinction to that which is mechanically mixed with the metal.

Dr. Bromeis effected this by dissolving the mixture in muriatic acid; the carbon, in chemical combination, unites with hydrogen, and forms carburetted hydrogen, while the carbon in mechanical mixture, takes no part in the action, but remains unaffected, and may be accurately determined. This quantity being subtracted from the whole carbon obtained by combustion, affords a means of estimating the quantity in chemical combination. Dr. Bromeis found in crystalline white cast iron, 3.8 per cent. of carbon. But as some white cast iron has been found to contain 4.2, or even 5.3 per cent., Dr. Bromeis considers that manganese may be substituted for it; he sometimes found as much as 7 per cent. of this metal. It appears, therefore, that neither common nor white cast iron are polycarburets of determinate constitution. In white cast iron Dr. Bromeis found only 0.5 per cent. of mechanically combined carbon, in other kinds nearly 1 per cent., and in grey cast iron, 2.3 per cent. Hence it follows, that the chemically combined carbon amounts only to 0.9 per cent.: Karston found 0.85 per cent.; cast steel, according to Gay-Lussac and Wilson, contains 0.93 per cent. of carbon. Bromeis found in hard cast steel 0.97 per cent. Grey cast iron may be considered as a mixture of very impure cast steel with carbon. This may possibly be the cause that it can be so easily hardened on the surface.

The next paper was by Mr. Richardson, "Contributions to the history of the Magnesian Limestones."—The author, considering the great importance of the magnesian limestones, both to the manufacturer and agriculturist, conceived that an account of their composition might prove acceptable. He examined the various limestones systematically, according to the excellent arrangements of Professor Sedgwick, and collected the results of his analyses in a tabular form. The insoluble residue of the specimens subjected to analysis, contained, in every case, organic matter. The analyses proved a very great variation in the quantities of lime and magnesia; a fact which will not surprise the chemist when he considers that they are isomorphous, and therefore capable of mutual replacement. Mr. Richardson argued, that the deposition of the lime and magnesia must have been effected simultaneously, from the fact of layers of limestone existing above and below the magnesian limestones, in which layers no magnesia can be detected. He was inclined to ascribe their deposition to the influx of waters holding chloride of magnesia in solution, which, meeting with calcareous matter held in solution by an excess of carbonic acid, robbed it of that excess, and the two carbonates of lime and magnesia fell together.

Dr. Kane remarked that Dr. Apjohn had examined some Irish Dolomites of the magnesian limestones, and had found the carbonates of lime and magnesia in atomic proportions. Mr. Croft stated, that he had observed the same fact in analyzing specimens from Saxony and other countries.

Mr. W. Snow Harris then made a Report on the Meteorological

Observations made at Plymouth last year. He stated that, at the close of 1842, he should be able to revise (and bring to the next meeting of the association) the results of the series of meteorological observations continued hourly, night and day, without material interruption, during ten years. He now submitted only a general discussion of five years' observation of the barometer, during the years 1837 to 1841, both inclusive, and some observations and experiments on the wind, made with Professor Whewell's anemometer. The observations were made at a height of seventy-five feet above the level of the sea, and were produced to 32° Fahrenheit. He exhibited a chart, showing the lines resulting from the means in each of these years, and also the mean of the whole five years, and noticed the surprising coincidence in the general character of all these lines, and the very few and small deviations they presented: a remarkable result, considering the frequent atmospheric disturbances to which these latitudes are liable. The mean pressure of the six years corresponded with that already obtained. The line of mean pressure occurred between the hours of 1 and 2, and between 7 and 8 A.M., and again between 12 and 1, and between 6 and 7 P.M. The hourly maximum pressure was at ten o'clock morning and night, being with only one exception, the uniform result for six years. The hourly minimum pressure occurred at 4 A.M. and P.M., being the uniform result for six years, without any exception. The line of mean pressure was crossed four times in the twenty-four hours; and thus was realized, in the midst of atmospheric disturbances of very considerable amount, that effect, termed "horary oscillation," which was first observed by Baron Humboldt, in tropical climates. Mr. Airy, to whom these observations were submitted, seemed to think, that but little more could be effected by a further continuance of them, after the close of this year. There had been 48,000 hourly observations on the atmospheric pressure, and 87,000 hourly observations on the temperature; and he trusted these would not be preserved merely in the fragile form of MS., but placed at the disposal of the scientific world. After explaining the construction of Whewell's anemometer, he said, that when the pencil tracing the integral effect of the wind, moved at the rate of one-tenth of an inch per hour, the current of air at the same time moved at a mean rate of eleven feet per second. He (Mr. Harris) had, by means of this instrument, endeavoured to arrive at something like an approximation to the velocity and direction of what he believed would amount to a trade wind. He had a table of results which gave the mean velocity of the wind (in feet per second) for each month of the year, viz :—

	Feet per second.		Feet per second.
April	13	October	15.29
May	12.6	November	14.96
June	10.9	December	12.54
July	9	January	12.76
August	12.87	February	13.97
September	15.42	March	14.63

So that the mean velocity of the wind during one year (leaving the direction out of the account), was about nine miles per hour. If the mean velocities arrived at in this table were diminished and made proportionate to the whole length of the wind, we should then have something like a general idea of the velocity of the aërial current as deduced from observation and inquiry. Then, according to Mr. Whewell's method of taking the observations (which he was persuaded was the only true method), in the latitude of Plymouth, they had something like a trade wind, setting in from southerly to northerly points of the compass at a mean velocity of four and a half to six miles in the hour. This was something like a definite result in meteorology; for no person before had ever attempted to discover the direction and velocity of the wind in its rate per hour, setting in a given direction. In these statements he had been dealing only with mean results.

Mr. Howard hoped that Mr. Harris would not think of discontinuing these observations until at least the cycle of eighteen years had been completed.—Dr. Scoresby would wish that observations with the anemometer should be tried on the sea, in order to get rid of the friction and other causes of retardation which affected the current of air over the land.—Col. Sykes believed that the hours of maximum and minimum mean pressure observed by Mr. Harris at Plymouth, would be found nearly, if not exactly, the same as those observed in India at an elevation of 2,000 feet above the sea, and those observed in Mexico by Humboldt, 10,000 feet above that level.—Col. Sabine said, he had that morning received a very important letter from Prof. Wheatstone, which had been laid before the Committee of the Section, and which they wished should be communicated to the Section previous to being transmitted to the Committee of Recommendations. It contained a proposal to make, for the Observatory at Kew, an apparatus which should record the operations of all meteorological instruments, so as to effect a great saving of cost. One of the instruments was for measuring the force and direction of the wind, and was capable of being sent up in captive balloons, so that the currents, to a height of 8,000 or 10,000 feet, might be carefully examined. It was stated in the letter that, all attempts to make self-recording thermometers, barometers, &c. by mechanical means have hitherto failed, because the mechanical force exerted by the rise of the mercury in the tubes is insufficient to overcome the friction of the attached mechanism, and only very in-

accurate indications can be obtained. The principle, however, observes Mr. Wheatstone, which I employ in my meteorological telegraph, viz. the determination (by means of a feeble electric current) of any required mechanical force, by the mere contact of the mercury in the tube with a fine platina wire, enables all these difficulties to be overcome. * * I propose, therefore, that such an instrument, the cost of which I estimate will not exceed £50, shall be constructed under my direction, for the Richmond Observatory. If, after a few months' trial at the Observatory, it shall be found to succeed, as I confidently expect it will, a great impediment to the advancement of meteorological science will be removed. Persons in almost every locality may be found who would not object to devote a few minutes per day to prepare such an instrument for use, but who would find it impossible to give the requisite attention to make hourly or half-hourly observations themselves.—Col. Sabine said, Mr. Bache, of Philadelphia, has requested me to explain the reasons which have prevented him from completing the Report on the Meteorology of the United States, which has been first called for, and which, at the meeting of the British Association at Newcastle, Mr. Bache, being then present, was requested to prepare. But the task of providing funds by private subscription in the United States for the support of the system of magnetical and meteorological observations recommended by the association (which is elsewhere provided for as a national work) has been undertaken by by Mr. Bache, in addition to that of superintending its establishment and progress. Being thus occupied, Mr. Bache has found himself unable to devote the necessary time and thought to the report.

Mr. Nasmyth prefaced his paper "On the Application of the Law of Definite Proportions to the Stratification of Clouds," with the remark, that he was first led to speculate on this subject, by observing the arrangement of clouds in fine weather, when, towards the horizon particularly, they may be seen extended in parallel bands or stripes. He conceived that the excess of vapour floating in the atmosphere beyond what the air could combine with, formed clouds; and that the air, in each electrical state, was capable of sustaining a definite proportion of vapour, and consequently, that the clouds of one class or description, floated (in what might be called a plane of equal electricity) at a uniform distance from the earth.

Prof. Bessell, Königsberg, made a communication "On the Astronomical Clock." Having ever been of the opinion, that that indispensable instrument to the astronomer, the transit clock, could only acquire perfection if the pendulum, separated from the works, were made to vibrate in equal time, whatever the temperature and the arc might be, he would submit, whether the expeditious method of coincidences might not be employed for checking the pendulum in both respects. The pendulum, apart from the clock, being suspended from the wall, a clock, taken out of its case, might be placed

before it at a distance of six or eight feet: an object glass three or four feet focal length, might be placed between both, so as to produce exactly at the lower end of the pendulum of the clock an image of the lower end of the other pendulum. Then the coincidences of both might be accurately observed by a telescope placed at a convenient distance. Similar contrivances had been described in an account of some pendulum experiments made at Königsberg; and the accuracy of the method was such, that the relative rate of both pendulums might be ascertained with sufficient accuracy in a very short time: in from ten to twenty minutes. The rate of the pendulum was to be tried at different temperatures, being placed in a box, having an opening at the lower end covered with glass, and so fastened to the wall that the pendulum could swing within it. In the construction of the pendulum, attention should be paid to one thing, which seemed to have been much overlooked. It often happened that thermometers affixed to the top and bottom of a clock case did not agree, whence it was evident that the compensation, acting only below, would not compensate for the variation of the whole rod. He should prefer, on this account, the gridiron to the mercurial pendulum, especially if the rods began as low as possible below the point of suspension, and were carried on to the centre of gravity of the lens. He should prefer the several rods to be of equal diameter, and to be coated uniformly. Supposing the spring perfectly regulated, as well with respect to heat as the arc, only one cause would interfere with regular vibration times. This was the effect of the part of the elasticity of the air which depends on the variation of the height of the barometer; the other part depending upon the variations of the thermometer, is comprised in the adjustment for the compensation for heat. There was a possibility of compensating the former, by fastening a barometer tube to the pendulum, and it would not be difficult to find the suitable diameter of the tube; but he was aware that this complication of the pendulum would be rather inconvenient. At all events, the variations of the barometer were not very great, especially if the specific gravity of the pendulum be made as great as possible. He submitted these hints to those celebrated artists, whose works had greatly contributed to the promotion of astronomical purposes, and the determination of the longitude.

Sir Thomas Brisbane said, that Dr. Robinson, of Armagh, had ascertained that a variation of the height of the barometer, amounting to one inch, produced a difference of $0''.27$ per diem of rate in in the clock. He observed, that the place at which the crutch impelled the pendulum, was a matter of more consequence than was usually supposed.—Mr. Dent denied that comparisons could be safely instituted between pendulums, and those connected with the mechanism of a clock.—In reply to a remark made by Mr. Clare, Professor Stevelly said, that cycloidal motion was laid aside on account of the shifting of the position of the centre of oscillation resulting from it. But, he thought, the discussion of compensations was too wide a departure from the important subject brought before the

Section by Professor Bessel.—Sir John Herschel acknowledged the justice of this remark The question before the Section was, whether the methods proposed really compensated for the unequal arcs of vibration of the pendulum, or not. Fortunately the method of coincidences reduced the labour of subjecting this to the test of actual trial: that which, a few years since, would have required days, or even months, to determine, could, by this method, be now ascertained in little more than twenty minutes.

Mr. Follet Osler "On the Application of the Principle of the Vernier to the Subdividing of Time."—Professor Stevelly said, Mr. Osler's idea was, to have a pendulum which should make, say ten swings, in the time that the principal pendulum made eleven, furnished with a small dial, and so placed that the coincidences, or want of coincidence, could be observed. The strokes of such a pendulum being counted, the time of every observed stroke of it, reckoned back from its coincidence with the principal, or seconds pendulum, would, it is obvious, be found in tenths of a second.

Sir David Brewster then made a communication "On a new Property of the Rays of the Spectrum, with Observations on the Explanation of it given by the Astronomer Royal, on the principles of the Undulatory Theory."—If we cover half the pupil of the eye with a thin plate of any transparent body, and thus view a prismatic spectrum, so that the rays which pass by the plate interfere with those which pass through it, the spectrum is seen crossed with beautiful black and nearly equidistant bands, whose breadth, generally speaking, increased with the thinness of the plate. If the edge dividing the ray were directed to the red end of the spectrum, then fringes were seen; but no such fringes appeared when it was turned to the violet end of the spectrum. One peculiarity of these fringes, not before noticed, was that they had not the forms of bands, but rather the appearance of screws, or dotted black lines, or as if they were formed by the shadow of a plate of metal perforated by small openings. This, which appeared to be a new property of light, and to indicate a polarity in the simple rays of light, when separated from each other by refraction, he had commented on at the meetings of the association at Liverpool and Bristol; and Mr. Airy, the Astronomer Royal, had given a paper and two publications on the subject, in which he endeavoured to account for this upon the undulatory theory, arguing that the appearance and magnitude of the fringe depended upon the diameter of the pupil, or of the object-glass. Sir D. Brewster said, he had repeated all his experiments under every variety of form, varying the diameter of the pupil from its greatest expansion to its greatest contraction, and the diameter of the object-glass from four inches to a quarter of an inch, and the fringe remained utterly unaffected by these variations. He further found, that these fringes varied in magnitude with the distance of the eye from the refracting body, and not with the magnitude of the pupil. He stated several other results, all of which, he thought, could not be explained on the principles of the undulatory theory.

Sir W. Hamilton observed, that the warmest advocate of the wave theory of light must be gratified with these valuable experiments of Sir David Brewster; even though they should require the wave theory, in its present form, to be abandoned; and yet it was probable they might suggest the very modifications which will adapt it to the enlargement of our knowledge.—Sir John Herschel said it appeared to him, that the wave theory was now placed in the same position in which gravitation was frequently placed in its infancy, when difficulties arose which could not be surmounted in the then state of mathematical knowledge. As soon as that knowledge was acquired, the theory was triumphant, or rather it was rendered more powerful by the very obstacles which it had been found capable of surmounting. The undulatory theory had already achieved so much, and even gone so far in predicting phenomena quite unlooked for, as in the extraordinary phenomena of conical refraction discovered by Professor Lloyd and Sir W. Hamilton, and in Fresnel's experiments, by which circular polarization was produced artificially, that he must protest against putting it on trial for life or death the moment a new fact was discovered which seemed inconsistent with it. The facts just brought forward by Sir D. Brewster were most extraordinary, and deserved the deepest consideration; but it was necessary to suspend our judgment until further inquiry, and perhaps new facts, threw more light on this very difficult subject. The screw-like appearance and dotted lines described by Sir D. Brewster, appeared to be one of the most extraordinary facts connected with the spectrum. But the spectrum might be said to be a world within itself, of which we know as nothing, compared with what remained to be known.—Sir D. Brewster did not wish to put the undulatory theory on its trial for life or death, but upon one count of the indictment; for he conceived it entirely failed in explaining those facts which he had brought before the Section.—Prof. M'Cullagh said, we as yet knew so little of the undulatory theory, that it would be premature to pronounce that it either could or could not explain every fact. It had long been his settled conviction, that it would be a foundation for physical principles. The only physical principle, in fact, which we had connected with it was that of interference, for he put no faith in Fresnel's mechanical theory of refraction, which seemed to be discovered by some kind of mathematical deduction, and then explained by principles invented to suit it. These were facts which he had long thought contradicted the fundamental principles of M. Cauchy as expressed in his fundamental equation. The well known fact of circular polarization, he conceived, contradicted it.—Prof. Lloyd agreed with Prof. M'Cullagh on the importance of establishing a sufficiency of physical principles, but he could not admit that the wave theory was so destitute of them as Prof. M'Cullagh represented it. Surely there was the principle of transversal undulations discovered by Dr. Young.—Prof. M'Cullagh thought that was rather a mathematical, than a physical theory, but, as he did not wish to differ about words,

he would admit it.—Prof. Lloyd said, it could be distinctly conceived as a physical principle. But if the theory was so destitute of physical principles, surely it was most wonderful that it was found to explain completely so many and such apparently unconnected facts, and this, he conceived, at least proved its parallelism with truth.—Sir W. Hamilton hoped it would not be supposed that the wave men were wavering, or that the undulatory theory was at all undulatory in their minds. The true practical question was, whether the dynamical explanations of M. Cauchy had a physical foundation, and, on that subject he knew no better authority than Prof. M'Cullagh. He was not so sanguine as to hope that, at a single bound, they should reach the physical conceptions connected with so subtle an element as light. He hoped Prof. M'Cullagh would publish his refutation of M. Cauchy's theory, particularly as it related to circular polarization.—The president said, that however difficult and abstruse the subject was which had been under discussion, the Section had had the opportunity of hearing the opinions of all the chief promoters of the science of optics in modern days. We were, perhaps, too impatient with reference to what this theory would or would not explain. We should recollect that it was a century and a half after Newton's publication of the theory of universal gravitation, before all the problems related to that theory, as, for instance, the problem of three bodies, could be brought entirely under its dominion. It was not more than fifty years since the undulatory theory was brought forward as anything more than a guess, and scarcely twenty since its principles were mathematically followed out to any considerable extent.

Sir John Herschel then read a "Report on the great co-operative system of Magnetic and Meteorological Observations," which, three or four years ago, was commenced at the instance of the British Association. After noticing the vast increase of the surveys and observations, owing to the number of foreign establishments entering into the concerted plan, the Report referred to the Antarctic expedition, taking it up where the Report of last year left it, at Hobart Town, in 1840. Captain Ross observed the November term (for observations) in 1840, at the Auckland Islands. On leaving those islands, his adopted course led him between the two southern magnetic foci. It seems probable that he was still to the eastward of the present locality of the greatest intensity. The full import of the observations made in this voyage is not yet known, but it is understood that intensities have been observed by Captain Ross $2\frac{1}{2}$ times greater than the minimum observed by him near St. Helena, on the outward voyage; and that the general aspect of the intensity observations would appear to place the centre of the principal isodynamic oval in a latitude somewhat exceeding 60° south. The nearest approach to the magnetic pole was in latitude 76° 12′, long. 164° east, the dip being 88° 40′. The intensity was here found to be less than in 47° south. The Admiralty (who had rendered

every service to these inquiries) had placed under the care of Col. Sabine the observations made on board each ship, the results of which were most satisfactory, as regarded the practicability of making accurate observations at sea; for out of 647 observations of this kind, made between London and the Cape, on board the Erebus, one only had been declared doubtful; while the observations taken by both ships exhibited a steady accordance, that could not be accidental, and might well be called beautiful. From these it would appear, if earlier observations are to be relied on, that the line of least intensity, in successive meridians, is travelling rapidly northward. The term of November, 1840, had been kept (by Capt. Ross) at the Auckland Islands; those of May and June, 1841, at Van Diemen's Land; that of July, at Sydney: the four succeeding terms had been kept in New Zealand. From a letter from Capt. Ross, dated 22nd November, 1841, it appeared the expedition was to sail the day following to resume the investigation; it was his intention to traverse the isodynamical oval, surrounding the focus of greatest intensity, supposed to be in lat. 60° south, long. 235° east, commencing in long. 210°, and lat. 52° or 53° south; and steering thence directly to the edge of the ice-pack, to make, on reaching it, for the point where the first year's exploration of the new continent (of Victoria) had terminated, and to pursue that barrier; the working out of which intention might, of course, involve another winter, spent within the Antarctic zone. Should it be otherwise, we might expect ere long to hear of his arrival at Falkland Islands; but, in the other alternative, another year would elapse without any further tidings of the expedition. As to *British and foreign observatories*, the British and Indian stations, except that at Aden, as well as the chief continental ones, had long been in full activity. The Russian government has been pre-eminent in the aid given. Supported by M. Cancrin, Minister of Finance, as well as aided by the funds placed at his disposal by Prince Mentchikoff, M. Kupffer had brought into activity magnetic observatories at Kasan, Barnaoul, Nertschinsk, and Catharineberg. He had also effected the re-erection of observatories at Tiflis and Nicolayeff, and the erection of a new one at Moscow, under the care of Count Strogonoff, curator of the university of that city. These operations, conducted by every European power, had occupied much time; the original term granted by our own government and the East India Company expired in the current year, just when the arrangements were completed over a great portion of the world, and the fruits were beginning to be gathered in. Accordingly, application was made to government by the President and Council of the Royal Society, for their continuation for another period of three years, to terminate in 1845; and at the same time it was officially stated, on the part of the Russian government, that the observatories in that empire should be kept up as long as the British ones, Baron Brunow stating, that this extension was the shortest term adequate to obtain results to repay the outlay. The British government gave an unhesitating assent to the continuation of the present scheme

for three additional years. For this new period the past had been an excellent preparation; all improvements that experience could suggest would be adopted; the correction for the temperature of the magnets, which is found to be the most important of all, will have been determined. But the past had not been merely a season of preparation, it had afforded demonstrations of the ubiquity of those singular disturbances called magnetic storms, which could not otherwise have been obtained, and data for the revision of the Gaussian theory. As to *magnetic surveys*, in South Africa, Lieut. Clarke, R.A., had joined the observatory at the Cape, as assistant to Capt. Wilmot; and it was proposed that the survey should comprehend, in addition to the colony, as extended a portion of the earth's surface, from the observatory, as circumstances would permit. The Admiralty had instructed the Admiral on the station to permit the sea portion of the survey to be carried into execution, so far as it was not prejudicial to the service, in her Majesty's vessels, and these surveys would include the coast on each side the Cape, and then we should be better able to judge of the expediency of completing the survey by an expedition into the interior. In North America, Lieutenant Lefroy, R.A., had been appointed to the principal observatory at Toronto; and was now in England preparing instruments. The Hudson's Bay Company had liberally undertaken to furnish conveyances in the years 1843-4 and 1845, to extend the surveys to the Pacific Ocean; and they also made an offer of passages on board their annual ships to England, and this would enable them to include in this magnetic survey Hudson's Bay and Straits. In the United States, Professor Bache (of Philadelphia), during the last summer, had completed the survey of Pennsylvania, commenced in the previous year, including three series of observations: the declination, inclination and intensity. Professor Loomis had extended his observations of inclination over a great part of Ohio, Indiani, Illinois, and Missouri. These, and numerous other observations and surveys in the States, would connect the northern British survey with the determinations of Capt. Barnett, of the Thunder, in the gulf of Mexico. As to *observations at sea*, by Mr. Fox's instrument, the inclination and dip of the magnetic intensity might be measured with all the precision requisite for every use to which observations at sea could be turned, for the purpose of tracing out the isodynamic and other magnetic curves in portions of the globe covered by water. To extend and facilitate the use of this valuable instrument, the set of instructions drawn up by Col. Sabine had been printed by order of the Admiralty, as a general circular, with some statements of the mode of using it practised on board the Erebus and Terror; and the hope was expressed that this method would be followed, not only in exploratory voyages, but by ships pursuing ordinary tracks, so as to furnish data for complete magnetic sea-charts. For these important observations, as well as the declination, it was necessary to eliminate the influence of the ship's iron: an evil increasing from the greater quantity of iron now

used. After mentioning the observations of Capt. Belcher, of the Sulphur, on more than twenty islands in the Pacific seas, which had arrived in England, and would be published, and the important results deduced from M. Erman's journey in Siberia, the report noticed the subject of *magnetic disturbances*, respecting which Gauss remarked, that one of the results of this great British enterprise was that the existence and extension of these disturbances over the whole of the globe had been ascertained. As a physical fact deeply connected with the general causes of terrestrial magnetism, this was indeed a result of the first magnitude; and considering all the circumstances, how it was modified by distance and locality, was eminently calculated to lead to theoretical truths. It distinguished what was local from what was general, and traced individual shocks from observatory to observatory, and station to station, till they were so far enfeebled as to be confounded and masked by the growing influence of other shocks nearer the principal point of observation. The report recommended smaller bars than those now in use, as more easily affected by sudden shocks. It was now considered advisable to collect from all sources to which we had access, accounts of the remarkable disturbances, beginning with 1840 and 1841, arranging them in chronological order, and publishing them in volumes by themselves, and the first volume would be published in the course of this summer. The great disturbance of the 25th of September, 1841, which was observed at Greenwich, and was immediately made the subject of a circular from the Astronomer Royal to his brother observers, was also observed at Toronto, St. Helena, the Cape of Good Hope, and Trevandrum in Travancore. All these arrived in time to be inserted in the volume for 1841; and surely it must be regarded as a remarkable fact, that this casual phenomenon was seized upon by our observers in Europe, Asia, Africa, and America, reported thence to England, reduced and printed in three months and one week after its occurrence: "tantum series juncturaque pollet." The returns from the different stations showed that these disturbances were general; that, though the movements individually might not be, and in fact were not, always simultaneous, the observations on the same day never failed to exhibit unusual discordances at all the stations, and were generally characterised by the diminution more or less of the horizontal intensity, prevailing more or less for several hours everywhere, and the movement of the north end of the needle towards the west. Besides the colonial observatories, these phenomena were watched with great attention at the observatories of Prague, Munich, and Greenwich. The report next noticed the new magnetic instruments and modes of observation. We can only enumerate the former, viz. the transportable magnetometer, Dr. Lloyd's induction inclinometer, Weber's inductive inclinometer, and another method proposed by Dr. Lamont. The report next enumerated the publication of various magnetic observations. The only expense incurred by the Association during the year, was £10 18s. 10d., for observa-

tory registers, and the committee prayed a continuance of their grant. Sir John Herschel stated, that the committee for revising the nomenclature of the stars deferred reporting till the catalogue of stars, now in preparation under the auspices of the Astronomical Society, was ready for publication; and that the committee for the reduction of meteorological observations, in consequence of the illness of Mr. Birt, had been unable to make any considerable progress.

The Rev. Dr. Scoresby, "On Improved Permanent Magnets, and the Modes of determining their Powers, with certain undescribed Phenomena in Permanent Magnets."—Dr. Scoresby exhibited a powerful, though moderately sized magnet, as a proof of the practical nature of his researches; and, after some explanations respecting the quality and temper of the metal in magnetized bars, he stated, that when his Researches (now in the press) on the Magnetic Power of the various denominations of Steel, &c., shall be completed, he will be able to determine, at once, and unequivocally, the proper kind and temper of steel, suited to a needle of any given size, and for any specific purpose.

Prof. Lloyd said, that Dr. Lamont, of Munich, was following out a similar course of investigation. M. Nobili had endeavoured to ascertain the constitution of permanent magnets.

Col. Sykes "On the Meteorology of the Province of Coorg in the Western Gâts of India."—The capital of Coorg stands at an absolute elevation of 4,500 feet, and the barometrical observations made there show the same hoary variation or semi-diurnal oscillation of the atmosphere which Humboldt had observed in South America, Col. Sykes in the Deccan, Col. Sabine on the coast of Africa, and which did not wholly disappear, though it diminished in amount, and became hard to detect amid increasing irregularities, in high latitudes.

Mr. Luke Howard produced a chart exhibiting, in curves, the chief meteorological changes observed by him during a cycle of eighteen years. The results of these observations will be found developed and explained in his work on the climate of London.

A letter from Dr. Lamont, director at the Observatory at Munich, was read, stating the names of a great number of professors in Germany, France, Italy, &c., who were aiding in making meteorological observations (commenced seven months ago), so as to present a complete series, extending over the vast tract of country between the Pyrenees and the Russian frontiers. The results are published in the *Annalen für Meteorologie, &c.*

Sir D. Brewster made a communication "On the Dichroism of the Palladio-chlorides of Potassium and Ammonium."—Dr. Wollaston had found that a long crystal of either of these salts, when looked through transversely, had a green colour, but when looked through from either end, had a red colour; and he (Sir D. Brewster) placed one of these long crystals transversely over another, in a cruciform shape, and then found that those portions of the centres of both, which were in contact, gave a red colour, while all the ends of the crystals were green.

The President then said that a letter had been placed in his hands from Professor Marianini, in Italian, which he had undertaken to translate for the Section; but the heavy duties which devolved upon him during the week completely prevented him. It contained a communication "On the Magnetic Action of Instantaneous Currents of Electricity."

(To be continued).

On the Electric Column. *By* J. A. De Luc, Esq., F.R.S.

(Continued from page 90.)

PART III.

Concerning some Meteorological Phenomena, to the better knowledge of which it may lead as an Aërial Electroscope.

When, in our researches, we have in view some great and determined object, we are not only more assiduous in our endeavours to approach it, but more attentive not to be misled in the road, and less disposed to be satisfied with mere surmises, while we perceive that some real discovery may be obtained by more circumspection. I shall therefore explain first, why every new electric phenomenon, which we encounter in the course of our experiments, must be attentively pursued and analysed in itself, and not connected with gratuitous hypotheses, for fear of losing a thread, which might lead us in the labyrinth of the physical causes acting on our globe, among which the electric fluid holds a high rank; as will appear by the following great object concerning this fluid, on which natural philosophers have not yet sufficiently fixed their attention, though it is explained in my former works.

It is commonly supposed that the electric fluid, which under the form of lightning darts from certain clouds, existed previously in them, ready to be discharged at a proper distance on bodies which possess less of this fluid, either other clouds or the ground. On this idea, not improbable at first sight, Dr. Franklin founded his invention of pointed conductors elevated above houses, in hopes to preserve the latter from being struck by thunderbolts. With the above supposition, this method of security was very ingenious; for, if the electric fluid were actually accumulated in a cloud, ready to be discharged on the first part of the ground sufficiently elevated, a pointed conductor might discharge that cloud without a spark, as it does the the prime conductor of an electric machine. But those who have frequently travelled on high mountains know certainly, that there is no analogy between a thunder cloud, and an insulated body on which electric fluid has been accumulated.

A cloud is a mere thick fog, and thus such a completely conducting medium, that the most powerful electric machine worked in it could not, for an instant, accumulate the electric fluid on its prime conductor; it would be constantly diffused through that moist air, and lost in the surrounding bodies. This cannot be doubted; but

it is supposed, that clouds, being surrounded by pure air, and thus insulated, can retain the electric fluid accumulated in them by whatever cause. In this consists the illusion, dissipated by what is observed on mountains. I have frequently been in valleys of the Alps, and of lower mountains, beset with thunder clouds leaning on both sides against wet grounds, and thus in so complete a conducting connexion with the mountains themselves, that it was impossible any accumulation of electric fluid could remain in the former; beside which, no cause of such an accumulation has ever been explained: however, flashes of lightning were emitted from these clouds, with greater or smaller intervals, followed by the astonishing phenomenon of the rolling of thunder; and to suppose this to be the repetition of one sound, by echoes from cloud to cloud, is a fiction similar to that of poets or painters, who represent the gods as as sitting on these fogs.

Lightning and thunder, when considered in their true nature, and with all their associated circumstances, though they are the most striking, have remained till now the most obscure of the atmospheric phenomena; and as at the same time their production is evidently connected with all the causes acting in the atmosphere, that great laboratory of nature on our globe, beginning from the very formation of clouds, this obscurity is spread over all the terrestrial phenomena. It is certain, by what I have above explained, that an instant before a flash of lightning strikes our eyes, no accumulation of electric fluid could have existed in clouds leaning against wet grounds: the sudden manifestation of this enormous quantity of electric fluid, not existing before as such, must therefore be the consequence of some chemical operation, depending on some new cause, which either disengages it from some combination, or generates it by some composition; and being thus instantly set free, it rushes in a torrent, before it can be diffused in the cloud and through this in the ground. Besides this immediate consequence of the certain fact, that the quantity of electric fluid thus emitted did not, the instant before, exist as disengaged in the cloud, various other phenomena attending this effect, prove the existence of some great successive chemical processes, manifested principally by the successive detonations forming what is called the rolling of thunder; these are undoubtedly produced by concomitant decompositions and recompositions of still unknown atmospheric fluids, some producing the decomposition of the air itself, others proceeding from this first operation, as shall be explained hereafter.

This is one of the greatest objects that could be offered to the attention of natural philosophers: for it must strike them, that no system on the nature of air and water can have any solidity, if it happens to be in opposition to these grand effects produced, under our inspection, in the great laboratory of nature: and though our observation has not yet extended to all the atmospheric phenomena necessary to be embraced for the discovery of their specific causes, yet it is sufficiently advanced to indicate, according to general

known laws, these decompositions and recompositions of atmospheric air, as being a fluid *sui generis*, and not a mixture of two aeriform fluids, differing in their nature, as has been concluded from specious phenomena produced in our experiments; but these phenŏmena I have explained in my works, without supposing such a mixture, in itself contrary to a number of atmospheric phenomena. This I shall here successively explain, though not with so many particulars as are contained in my works.

Rain will be my first object; and indeed it ought to be so in every general system of chemistry, since no phenomenon, either spontaneous or artificially produced, is more connected with the manifestation of water in the modifications of expansible fluids; and none certainly is attended with greater consequences on our globe. With a view of supporting the new hypothesis of a certain composition of water, from which and its associate hypothesis of two distinct and defined aeriform fluids in the atmosphere, rain, so common a phenomenon, cannot be explained, the ancient and already exploded hypothesis of M. Le Roy, of evaporation being a dissolution of water by air, has been revived. This hypothesis, the only apparent resource of the modern theory of chemistry, was plausible at the time of its first publication, about sixty years ago, when meteorological observations were very little advanced; because it is certain, that evaporation restores, upon the whole, to the atmosphere, the same quantity of water as falls from it in rain, dew, and other aqueous meteors: but from a number of well determined phenomena, discovered by the progress of observation, this compensation is not immediate: that water, which ascends in the atmosphere by evaporation, passes through an intermediate state, undergoing a chemical transmutation, which makes it disappear to all our tests, sometimes for many months, it being then transformed into an aeriform fluid; and it must be by some inverse operation, that, all at once, clouds, rain, and the other concomitant phenomena are produced. I shall show hereafter how unfounded, as well as useless, is an hypothesis imagined for evading the consequences of these phenomena, which I have opposed to the new theory of chemistry; but first, I must proceed farther in the account of the phenomena themselves.

The above consequences may be deduced from the most common atmospheric phenomena, even when only viewed from the plain, provided they are observed in all their consequences; but it is on high mountains, the very region of meteors, that, from other circumstances not perceptible in lower situations, the observer is induced to wish for more knowledge in the astonishing operations performed in this laboratory. Such has been the case with M. de Saussure and myself, on account of our frequent visits to the mountains of our native country, for the geological pursuits in which we were engaged at the same time. The surprising phenomena concerning moisture, which we observed in these high regions of the air, led us separately to the pursuit and construction of our respective hygrometers; in order to understand, by experiments and observations with

this instrument, in what really consists moisture in the atmosphere; and to follow certain of its modifications, as its sudden increase and diminution without perceptible cause: a knowledge which, if not leading to immediate discoveries on the other atmospheric operations, might at least clear the way to these discoveries by dispelling and preventing errors.

When our experiments and observations were first published, they attracted much the attention of natural philosophers; but by degrees they have been forgotten, from the increasing prevalence of the hypothesis of a composition of water to which they were opposed, in consequence of their connexion with the most common meteorological phenomena; an opposition explained even before this hypothesis was so much relied upon as to effect a change in the whole nomenclature and language of chemistry.

This inattention, for a time, to real and important discoveries, an effect occasioned by prevailing prejudices, is observed under various forms in the history of sciences; but there it is seen also, that an obstacle of this nature could not be perpetual, and it may be expected, that it will not be so in this case; therefore I shall here assemble some uncontroverted results of observation and experience, for the consideration of natural philosophers.

Article I. Evaporation, the original source of atmospheric phenomena, is not a dissolution of water by air, as is now so commonly assumed; air has no share in it. The immediate product of evaporation in all its stages, from the formation of steam by boiling water, down to the evaporation of ice in winter, is constantly and uniformly an expansible fluid, composed of water and fire, namely the aqueous vapour. This fluid, in whatever temperature it is produced, acts by pressure, in the same manner as the aeriform fluids, and in particular on the manometer, from the instant of its production, as long as it subsists; and the quantity of its production, attended with a proportional pressure, is the same in vacuo as in air, at its different maxima correspondent to each degree of temperature; a direct proof that air has not the smallest share in evaporation. Lastly, as long as this fluid subsists without any change in its nature, it never ceases to act upon the hygrometer, and its quantity is exactly measured by this instrument, with the addition of the thermometer. I have proved these assertions by the union of M. de Saussure's experiments and mine, in some papers published in the *Phil. Trans.* of 1793. It is evident, that, if these be real facts, the resource of the new theory of chemistry for explaining rain is overturned (as will be seen hereafter), and with it the theory itself; what then is the reason, that those, who still maintain it, remain silent on these facts? On this however rests (and will continue to rest till the contrary be proved by direct experiments) the whole of meteorology.

Art. II. Both M. de Saussure and myself have determined, by direct experiments related in our respective works, as I shall more particularly express hereafter, the quantities of evaporated water contained in one cubic foot of air correspondent to every degree of

our hygrometers, at every temperature; and we have proved, that the maximum of this water, a quantity fixed for every temperature, cannot be exceeded, either by the increase of water in the same space, or by the diminution of heat with the same quantity of this water, without some of the aqueous vapour being decomposed, and water making its appearance by precipatation: and by my experiments it is moreover demonstrated, that no length of time after the production of this fluid, can prevent either its effect on the hygrometer, or its remaining submitted in the same manner to the influence of temperature.

Art. III. The aqueous vapour, *i.e.*, the immediate product of evaporation, is therefore never concealed in the atmosphere; and its quantity, in any part of the latter, can always be determined by the observation of the hygrometer and the thermometer. This fluid, produced by the evaporation that never ceases on the surface of the water and of the land, being of a specific gravity less than that of air, constantly ascends in the atmosphere, passing through its lower regions, where we do not find that it remains; it ought, therefore, to accumulate in the higher parts. Now, as we ascend on mountains, the hygrometer indicates less and less evaporated water in the transparent air. I shall soon answer the hypothesis already mentioned, as having been imagined for setting aside the conclusion which I have deduced from this phenomenon, namely, a transmutation of the aqueous vapour into atmospheric air; a conclusion, however, which will be found the ultimate result of this series of facts

Art. IV. Another phenomenon, which M. de Saussure and myself have observed, proves that dryness is still greater in the region of the atmosphere above the highest mountains, where it was natural to suppose, and I supposed it at first, that the aqueous vapour was accumulating. On plains and small hills, moisture is increasing in the air after sunset; and before we possessed our hygrometers, we had reason to suppose that it was the same upon high mountains, for there also the grass becomes wet. This being the first common symptom of moisture observed after sunset, and even before, was one of the arguments in favour of the idea that dew proceeds from the ground; but the hygrometer, that neglected instrument, has shown it to be a phenomenon belonging to the physiology of plants, and not to meteorology. On high mountains, while the grass on the ground becomes wet, the hygrometer being suspended at some height above the ground, in some insulated spot where the air is free, shows an increase of dryness, which continues during the night. I have determined the cause of this phenomenon by immmediate observations; it proceeds from the condensation of the columns of air, while the heat diminishes in them; whence results that the part of that air which, during the day, rested on the summits of mountains, descending lower, is followed by the air which was higher before; and this, as long as the condensation continues in the lower parts, descending from higher regions, and thus passing over the

summits in its way downwards, is found, in an increasing degree, drier than that which rested on them in the day.

Art. V. Among the atmospheric phenomena, that of dew, commonly considered as very simple, has been long, and is still now, an object of controversy among natural philosophers who have not attended to the latest experiments and observations. The first and most plausible explanation was, that the dew descended from the air by the condensation of the evaporated water spread in it, when heat diminishes; but some experimental philosopher, finding that this cause was not sufficient to explain all the circumstances of dew, conceived the idea, which I have above mentioned, that it ascends from the ground, because this retains longer the heat of the day than the air above it, which circumstance was considered as increasing evaporation; both parties alleging in support of their opinion certain facts which, though not denied, were not decisive. During the most active time of this controversy, about sixty years ago, I made with my brother various kinds of experiments and observations, which, by turns, favoured one or the other of these hypotheses, but neither of them decisively; and the question would have remained for ever in suspense, had not hygrology and hygrometry been pursued with the degree of attention and labour that M. de Saussure and myself have bestowed upon them; from which the phenomena of dew has appeared under a new and quite different aspect, which excludes both the above causes as fundamental in it, and shows why neither of them could explain its most essential circumstances.

Art. VI. With respect to the experimental part, we have both determined, by direct and unconcerted experiments, the effects produced on our respective hygrometers, placed in a mass of air, wherein the quantity of evaporated water remaining the same, there was no change but in the degree of heat. We have made the same kind of experiments on different quantities of evaporated water in the same space; and combining them, we have formed tables expressing the different effects of heat on moisture, correspondent to different quantities of evaporated water in the same space, and to the changes of heat in each of these quantities; from which tables, after having observed the hygrometer and the thermometer in any part of the atmosphere, the quantity of evaporated water contained in one cubic foot of that air is determined. These entirely distinct experiments have proved the constancy of the laws prevailing in these effects, by the astonishing agreement of our tables, though determined by very different instruments and processes: an agreement which I have shown in the already mentioned papers to the Royal Society.

Art. VII. This determination of the effect produced on moisture, *i.e.*, on the indications of the hygrometer by the changes of heat, in a mass of air wherein the quantity of evaporated water remained the same, was most essential in meteorology; and in particular it was indispensable for the decision of the question, whether the production of dew were principally owing to the cooling of the atmos-

phere; which appeared the most natural explanation, but on which however there were sufficient reasons of doubt to produce the obscurity which remained on this phenemenon; because nothing could be either determined or proved, concerning the real effect of the diminution of heat on evaporated water, without such experiments as above defined; and I come now to their immediate application to the phenomenon of dew, in consequence of some observations which were also separately made by M. de Saussure and myself. Towards sunset and in the beginning of the night, moisture increases in the air much more rapidly; and after sunrise and in the first part of the day, dryness increases also much more rapidly; in both cases comparatively with the correspondent changes of heat, than would be the case did the same quantity of evaporated water remain in the air. This is a very succinct account of our experiments and observations concerning this object, the particulars of which may be seen in our respective works; but it is sufficiently distinct to allow me here to conclude, that thus has been pointed out one of the greatest questions and objects of investigation, concerning terrestrial physics, namely: what is the cause of the disappearance in the atmosphere of the greatest part of the aqueous vapour which it before contained, when the sun ascends on the horizon, and of the increase of its quantity when the sun is setting?—while the very reverse should have been expected from all the hitherto known causes, as I shall show hereafter. To this investigation I shall now proceed as far as known phenomenon will lead me.

Art. VIII. I shall first mention a very important course of observations of M. de Saussure concerning the changes in the electric state of the atmosphere. He had erected a high conductor, in a favourable situation, on the brow of a hill in Geneva. The lower part of this conductor was connected with an insulated pair of pith balls, the divergences of which indicated the differences between the electric state of the upper air and that in which the balls stood: he observed during many years the diurnal variations of this difference; and the main result of these observations is the following. In common weather, *i.e.*, when no particular cause disturbs the course of the usual operations going on in the atmosphere during each period of twenty-four hours, the quantity of electric fluid increases in it from sunrise till some time in the afternoon, as is seen by the increase of a positive divergence of the balls. The new electric fluid, the formation of which is thus indicated, accumulates in the air, because it is transmitted but slowly to its lower part near the ground. But afterward, when the hygrometer shows a beginning of increase of moisture in the atmosphere, the divergence of the balls begins to decrease; and when at last dew is forming, the electric equilibrium is soon established between its upper and lower parts, the whole of the electric fluid formed in the day passing then into the ground. Now, it is during the first of these periods, that dryness increases in the atmosphere much more than would happen by the same increase

of heat, did only the same quantity of aqueous vapour subsist in it as before sunrise; while on the contrary, its quantity ought to increase by a greater evaporation being produced on the ground, which dries when heated by the sun. Hence it appears, that there is some connexion between the increase of the quantity of electric fluid and the diminution of that of aqueous vapour in the atmosphere, during this period.

Art. IX. This points out, in the first place, a formation of electric fluid in the atmosphere, while the sun's rays pervade it. Light, the increase of which in the atmosphere is here the immediate cause, is certainly one of the component parts of the electric fluid; therefore, this fluid must be composed in some operations of nature on our globe. Now it is here already probable, that the sun's rays, in pervading the atmosphere, encounter in it the substances with which they compose the new quantity of electric fluid then manifested; and that, in general, they enter there into various combinations, is proved by their intensity being sensibly greater on the top of high mountains than in the lower parts of the atmosphere, as has been shown from experiments by M. de Saussure; which difference must proceed from their quantity being diminished in pervading the air.

Art. X. Some other experiments of M. de Saussure lead besides directly to this system concerning compositions and decompositions of electric fluid, as producing phenomena, the causes of which were unknown or mistaken. For instance, it has been found by experience that, when water is poured upon an isulated plate of hot iron connected with an electroscope, this plate becomes negative: whence it had been concluded that, when water is converted into vapour, it requires a greater capacity for electric fluid; and thus deprives of a certain quantity of this fluid the body on which it evaporates. But M. de Saussure having repeated the same process upon different heated bodies, found that some, in particular silver, became positive: whence he concluded very naturally, that during the evaporation of water on hot iron some electric fluid was decomposed, and some on the contrary composed when the same operation took place on silver. He has also surmised, what I have since found by direct experiments related in my work *Idées sur la Météorologie*, that in the discharge of the Leyden vial, and in my experiment of the magic picture, the spark produces some diminution of the quantity of electric fluid on these bodies; which cannot be but by decomposition. It will successively be seen in what manner these previous remarks on the electric fluid, and the experiments on the same subject contained in my former paper and the first parts of this, are connected with meteorology.

Art. XI. I have said that, as we ascend mountains, the hygrometer, successively falling, indicates less and less evaporated water in the air. We thus, however, attain the region in which clouds and rain are formed; and there it is, that the lessons of nature itself may guard us against the arbitrary dictates of imigination: I shall therefore relate what I have observed. At times when the atmosphere is

so clear, that distant objects are seen very distinctly, and that the hygrometer, according to the tables that M. de Saussure and I have made from direct and separate experiments, does not indicate above two or three grains in each cubic foot of that air, small clouds may be seen forming in all parts of the very stratum of the atmosphere in which we stand, with very little or no wind. Sometimes without any change in the temperature or moisture of the intermediate parts, these embryoes of clouds dissipate: but at other times they rapidly increase, unite together in the whole stratum in sight, and announce to the observer, that soon he will be enveloped by clouds. However, till the clouds, either moving towards him, or forming around him, occupy the very spot in which stand the hygrometer and the thermometer, he observes no sensible change in them: but the instant that a cloud envelopes him the hygrometer arrives at its point of extreme moisture, and all the bodies are wet.

Art. XII. These preliminaries of rain often remain a long time, with only some variations, and at last dissipate without effect; and as soon as the clouds disappear in one spot, the hygrometer indicates the same dryness, as if no cloud had been there. But at last; though without any perceptible difference in the preliminaries, because some other test of the state of the air, besides those we possess, is wanting, the clouds increase in extent and thickness, above and below the place of observation, and rain is produced in more or less abundance. If rain be lasting, and at the same time in a great extent of country, it may happen either in a calm air, or during some regular wind. But when rain is partial and in showers, sudden, and sometimes violent winds accompany these, arising from the expansion of the air, by its decomposition into aqueous vapour in some place, while a vacuum is produced in other parts by the resolution of that vapour into rain. Hence it is, that the direction of these winds is rapidly changing, and that they cease with the return of the transparency of the air. Lastly, in a stratum of air, which perhaps only half an hour before was calm and transparent, in which the hygrometer did not indicate any increase in the small quantity of evaporated water, and without any indication of increase of the quantity of electric fluid, some clouds, rapidly forming, produce lightning, thunder, hail, torrents of rain, and such violent winds, as tear up trees and overturn cottages on mountains.

We may be for ever ignorant of the causes of these wonderful phenomena, but those who are aware that fiction, in the operations of nature, may lead to great errors, will prefer ignorance to a false science. As for me, from my first observations of these operations of unconstrained nature, and with the addition of a remark of M. de Saussure, which I shall mention, I changed very essentially my former ideas on the atmospheric phenomena, as I have explained in my works, and shall repeat hereafter.

Art. XIII. In order to evade the general consequence which, in my works, I have deduced from these facts, namely, that rain and

the other concomitant phenomena are produced by different kinds of decompositions of the atmospheric air, which consequence is certainly the subversion of the new theory of chemistry, M. Fourcroy invented the hypothesis of a dry solution of water by air; supposing that this water could no longer affect the hygrometer, which, in consequence he discarded from the rank of a meteorological instrument, and having obtained the assent of many chemists who have not applied to meteorology any more than himself, this instrument, so much wished before by natural philosophers, is now hardly mentioned.

But this hypothesis, grafted on that of Le Roy, is in the first place absolutely gratuitous; no fact having been adduced in bringing it forward in chemistry against the positive facts contained in M. de Saussure's works and mine: and besides it is of no avail, since M. Fourcroy himself, and all those who have adopted it, have been obliged to suppose that this pretended solution of the water remains dependent on the temperature; which they are obliged to do, otherwise it would be nothing more than my system, with the appearance of refuting it. For, if the enormous quantity of water which sometimes falls in rain from a very limited stratum of air, be not submitted to precipitation by the diminution of heat, it must have been changed into a permanent or aeriform fluid; and in the atmosphere no sensible quantity of any fluid of this kind exists, but the atmospheric air. Besides, since for this reason it is supposed in that hypothesis, that the evaporated water remains dependent on temperature, very little knowledge in hygrology is required to conclude, that it cannot cease to affect the hygrometer in proportion to its quantity, as is evident from M. de Saussure's experiments and mine. Lastly, with respect to that fluid the decomposition of which produces rain, its nature is clearly determined by the following circumstance: when we remain in a stratum of air till the end of the operations by which a deluge of rain, even with lightning and thunder, has been produced, the residuum, according to all tests, is the same air as before. Such are the objections which I have made to M. Fourcroy himself, and which he has not answered, nor any chemist for him.

Art. XIV. These formations and modifications of clouds, when viewed only over head from the plain, have naturally inspired the idea, that by some cause the liberation and condensation of evaporated water now and then take place in a great extent of the upper region of the atmosphere, which water descends and accumulates in the stratum of air where clouds form and produce rain. But this idea proceeds from a want of previous knowledge in hygrology, and of observations on high mountains; for, in the first place, whenceever and from whatever cause that quantity of water may be supposed to proceed before any precipitation can take place, even in the first state of vesicular vapour which constitutes clouds, it must be preceded by extreme moisture in the still transparent air, since it is only the excess of that water, which is first precipitated in a mist;

and when this precipitation ceases, extreme moisture still subsists in the air, as M. de Saussure and myself have found in all our hygroscopical experiments. Now, I have said above, from observations on high mountains, that air is there dry till the moment before the formation of clouds, and that as soon as the clouds are dissipated, the hygrometer indicates the same dryness as before. This evidently shows, that the production of clouds and rain have their cause in the very stratum of air where they are manifested; and this cause cannot be any other than a decomposition of the air itself.

Lastly, in these very clouds, which, being themselves a conducting mass, lean besides against mountains, it happens sometimes that lightning and thunder are produced; and this, as I have said before, without any previous sign of an uncommon quantity of electric fluid in them. This also points out some operation taking place in these clouds, by some modification in the cause which commonly produces a simple rain. The electric fluid thus suddenly disengaged must have been before in some chemical combination in the air itself, which prevented its manifestation, and is then destroyed. When we are above the clouds, we may see (as it has happened to me) lightning darting upwards, as it is commonly seen darting downwards when we are under the clouds; and even in this last case, we may judge that lightning is darted upwards, when we see only a great sudden light in the clouds, without any flash, followed however by thunder.

The above are leading facts in the maze of atmospheric phenomena, certainly indicating the existence in the atmosphere of subtle fluids besides those which have hitherto been discovered. This is the general object which I am going to examine.

I shall here begin by explaining one of the results of my long labours in the pursuit of the measurement of heights by the barometer, of which all the steps are described in my work *Researches sur les Modifications de l'Atmosphere*, published in 1772. My experiments and observations were first directed towards these two points. 1. To obtain, by a great number of observations at different measured heights, on mountains and towers, a coefficient expressive of height, to the determined law of the densities of air correspondent to the difference of pressure in a given temperature of the air. 2. An equation for the differences of actual temperatures with that fixed point. By dint of trials, I arrived so far in these determinations, that the method of measuring the height of mountains by the barometer has been found preferable to the geometrical operations, on account of the impossibility of determining a law in the terrestrial refractions; besides the difficulty of finding proper bases for the triangles. But though the measurement of heights had been my first view in this undertaking, other modifications of the atmosphere became soon predominant in my pursuit, as I shall now explain.

In order to ascertain the degree of exactness, which could be ob-

tained in the determination of the two parts of the formula above defined, I had measured the heights of fourteen points above one another on Mount Salève, near Geneva; the whole height above the level of the base that I had chosen being above 3,000 feet; and at each of these determined elevations I had made a great number of observations of the barometer in different temperatures, both in the same days and in different seasons. I had taken all possible precautions to ascertain the height of each of these points, in verifying the trigonometrical operation by levelling the whole mountain in passing by these points, in order that the formula might be applicable to other places: but had there been some inaccuracy in that respect, it could not effect the coefficient of the two laws, as applied to the same places; for if these laws had been sufficient, the formula would have assigned to them the same height by every observation. Now, no coefficient to the differences of heights in the barometer, associated with any equation for the differences of the thermometer, could bring the formula to express the same differences of height between the same points: a proof that the two conditions, with which alone the formula corresponds, do not embrace all the causes of variation in the density of air. Having, however, no other data, I fixed these two parts of the formula in the manner the most correspondent to the whole of my observations, amounting to near 600; so that the sums of anomalies in plus and minus comparatively to the measured heights were equal; after which the causes of these anomalies became the object of my researches.

One of the principal means by which I had considerably reduced the former great anomalies in this measurement, which had appeared unconquerable, had been by introducing an equation for the differences of expansion of air produced by heat in the atmosphere. Considering therefore this effect of heat, which by its increase diminishes the pressure of columns of air of a determined height, under the same pressure of superincumbent air, as indicated by the height of the barometer at the upper station; and connecting this circumstance with the idea, that the cause of heat is an expansible fluid, namely free fire, which occupies in air a space without any sensible addition to its mass; I concluded that some other fluid, for which we had not yet a test, as we have by the thermometer for the former, might be the cause of the above remaining anomalies.

This general conclusion brought into my mind the aqueous vapour, of which I knew that the specific gravity was much less than that of air; and supposing at that time, as was commonly thought, that its accumulation in the atmosphere was the cause of rain, I conceived that the difference of its quantity in different times must be very great, and that this might be the cause, or at least one of the causes of the anomalies I had in view.

The same consideration led me also to a system concerning the remarkable, though not constant, correspondence of the variations in the sedentary barometer with rain and fine weather; as the same

fluid, the abundance of which in the atmosphere diminishes its pressure upon the barometer, was supposed to produce rain. Having published this system in the above mentioned work, it obtained much approbation among natural philosophers, because no satisfactory explanation had been yet given of the above connexion of phenomena; however, I did not intend to give it a full assent, till I had succeeded in the construction of a true hygrometer; judging already, that, without such an instrument, nothing could be determined with any certainty concerning the modifications of evaporated water in the atmosphere. This judgment was soon confirmed: for M. de Saussure, who had made a quicker progress than myself in hygrometrical experiments, discovered the fallacy of the above plausible system, which at first he had adopted with applause. He demonstrated by direct processes, that though the aqueous vapour is specifically lighter than air, the difference between its greatest and smallest quantity in the atmosphere at any time is so little, that it can explain but a very inconsiderable part of the variations of the barometer. I had not yet carried my own experiments so far, but I did not doubt the main result of his, as they bore all the characters of a true inquiry, and I abandoned my system, as applied to aqueous vapour.

But the general conclusion that I had deduced from the great reduction of anomalies in the measurement of heights by the barometer, which was principally owing to the introduction of an equation for the differences of the quantity of free fire in the air, still remained; namely, that some expansible fluids as imponderable as the former, hitherto unknown to us, might also account for that remarkable correspondence between the changes of the weather, and the variations of the barometer. For, the same fluids, which, from their abundance at certain times, lessen the pressure of the atmospheric columns on a certain extent of country, by dilating the air and repelling it to other parts, may also prepare its decomposition for the production of rain, either alone, or accompanied with other meteors; and at other times they may be dissipated without producing any of these effects, occasioning only the fall of the barometer.

The above series of facts and their immediate consequences present the greatest assemblage of operations of physical causes on our globe; and a general consequence certainly results from them, namely, that all these operations are so intimately connected with the nature of aeriform fluids, of water, light, fire, and electric fluid, that we cannot determine any thing with the smallest degree of certainty on the nature of any of these substances, without embracing the whole. When therefore we discover some new phenomenon of any of these fluids, at what distance soever this phenomenon may be from connecting itself with the operations which we observe in the atmosphere, it is not to be neglected; for we cannot arrive at any distant object, but by successive steps.

This is the consideration that has induced me to fix my attention on the electric phenomena manifested by the instrument, which I

have described under the name of aerial electroscope: as from the above atmospheric phenomena concerning lightning and thunder which cannot leave any doubt, that they are produced by a certain decomposition of the atmospheric air; and from the correspondent circumstance of a formation of new electric fluid in the atmosphere, during the period of the day when, the greatest part of the aqueous vapour vanishing in it, there remains scarcely any ponderable fluid but atmospheric air; it is manifest, that the electric fluid is one of the substances most intimately concerned in the chemical processes which take place in the atmosphere, and on the nature of which it is the most important to acquire more knowledge.

It may be seen in the tables of my observations of this new instrument, that the changes in the frequency of the strikings of the little pendulum have no determined connexion with those of either heat or moisture in the room; for though heat commonly increases in the course of each day, at the same time as the frequency of the strikings, nevertheless the former is not the cause of the latter; since with the same degrees of heat the frequency of the strikings is very different on different days. With respect to the correspondence of this phenomenon with the variations of the barometer, my observations have been too short for deciding any thing on this subject, though I felt much interested in it; and besides, the barometer had but small variations during this short time, being always rather high. Therefore, this is a course of correspondent observations which remains to be followed.

The observations contained in the last table create a new interest in this pursuit, as they may become a mean of discovering the changes in the comparative electric states of the ground and the air near it. The little pendulum, by its silver wire, being placed in connection with the zinc side in these experiments, was therefore positive; and in this case (as well as when it is connected with the copper side) it must rise more rapidly towards the ball 18 in proportion as the electric state of the latter differs more from its own. We know that in the first case (that of my observations), when the ball 18 communicates with the copper extremity of the columns, it is negative, and thus differs from the pendulum as negative from positive; the standard of which, according to the important determination of Sig. Volta, is the actual electric state of the ambient air. Now, the observations contained in the above table show that the frequency of the strikings is not always the greatest, when the ball 18 is undoubtedly negative, by communicating with the copper side of the columns; it being often equal, and sometimes even greater, when the ball communicates with the ground. This is a remarkable phenomenon, showing, that sometimes the ground contains less electric fluid than the air near it, and it may in future lead to some important discovery concerning the operations going on at the surface of the ground depending on the atmosphere.

I have employed much time and labour to arrive at the entrance

of this new road in the investigation of terrestrial phenomena: the entrance, I say, for I do not even consider it as completely open. With respect to the instrument itself, I may judge that it is susceptible of farther improvements, both in the composition of the column, and in the machinery added to it; for in such a complication of new physical effects and mechanical dispositions of parts, it is not to be expected that every thing can be conceived by one individual. The very composition of the column might be improved with regard to the intensity of effect by some other metallic coating than that of copper on paper, which I have employed on account only of its being ready prepared by using Dutch gilt paper. In some trials, I have found more effect in using paper covered with real gold, and with silver; and I have also found some advantage in doubling the Dutch gilt paper, by pasting a thin paper over its own paper. Many such trials may be made with a proper condenser, before whole columns are composed. As for the arrangement of the machinery connected with the column, the instrument which I have described having been successively augmented upon its original base, I suspect that it is too much crowded, and that thus its parts may have on each other an influence prejudicial to the effects, which I have marked in other cases.

I have made many other remarks on this instrument, but my present purpose is more to engage other experimental philosophers in this pursuit, than to forward it myself: for with respect to these observations, I consider them as newly born. It will first require some time for the understanding what may be called the language of the instrument; *i.e.* its meaning as to the indication of the electric state of the ambient air, by its influence on the motions of the pendulum. This study has been opened too late for me, though I was engaged in it by considerations resulting from long meteorological observations, which, as they are of the greatest importance to natural philosophy, must be the incitement to this pursuit. Wishing therefore, that such observations may become a more general object of attention among natural philosophers, I have here endeavoured to show, by an abstract view of their present results, what knowledge, in following them, may be still obtained concerning the atmospheric operations. It is true, that observations of this kind require the neighbourhood of mountains (unless those who ascend in balloons should carry proper meteorological instruments, and apply themselves to these observations), but in general no real knowledge of the nature of the atmosphere can be obtained without in some manner, ascending in it; and it is no less certain, that without this knowledge no chemical theory can possess any certainty.

Systems are useful for promoting science, provided they be founded on all the knowledge already acquired respecting their object; but even then, as long as they contain hypotheses, they must be only considered as leading to new researches on determined

points. With this view, I shall here conclude by an abstract of a meteorological system which I have fully explained in my former works, and especially in that under the title of *Introduction à la Physique terrestre par les Fluides expansibles.*

I. During the time that the sun's rays pervade the atmosphere, the aqueous vapour ascending in it by the evaporation which continually takes place on the surface of the earth, is transformed into atmospheric air by some combination of this vapour with the electric fluid, which, during the same time is formed in the atmosphere. A formation of electric fluid at that time is shown by M. de Saussure's observations already mentioned; but that the quantity thus manifested is not the whole, and that a great part of this new fluid is employed in the above transformation, is proved, as will be seen hereafter, by the production of lightning and thunder, which cannot have any other source.

II. Thus, but by a particular operation, is formed that subtle fluid which I have called vector, possessing many of the properties of light, but with the characteristic differences which I have determined. This fluid pervades instantly all bodies, is constantly present in the atmosphere, and has probably a great share in its phenomena; but its only function yet determined is, to unite with the electric matter composed at the same time; and, being thus the cause of the expansibility of the electric fluid, it produces the phenomena known under the name of electric influences, as I have explained.

III. In clear weather dew is produced at sunset, because that formation of electric fluid then ceasing in the atmosphere, the aqueous vapour, which continues to ascend in it, remains unchanged, and its quantity increasing too much in the air comparatively to the decreasing heat, it precipitates in visible particles of water: when heat decreases very rapidly in the air after sunset, the vapour is seen condensed as a mist over meadows; and at last in autumn it produces fogs.

IV. The return of atmospheric air into aqueous vapour, whence result clouds, and afterwards rain, is produced by some subtle fluid ascending from the base of the atmosphere, the affinities of which with the ingredients whereby the aqueous vapour has been transformed into atmospheric air decompose the latter. Thus, particles of aqueous vapour being substituted for particles of air in some stratum of the atmosphere, and becoming much too abundant to subsist in the same space, they first precipitate in the vesiculæ which form clouds; and if the decomposition of the air continues some time in the same stratum, these vesiculæ collapse into drops, and form rain.

V. This is one of the causes of the variation of the barometer, not as a prognostic, but as a consequence. The absolute mass of the atmosphere is constantly changing by these inverse operations. When there is a long duration of fine weather over a great extent of country, the absolute quantity of air increases in the atmosphere, by the aqueous vapour which ascends in it continuing to be transformed

into air during the day; and the barometer ascends, even in parts at some distance where it rains; when on the contrary there prevails over a great extent of country a long continuance of decomposition of air into rain, the mass of the atmosphere decreases, and the barometer falls, even in adjacent countries where there is fine weather. It is not therefore to be expected, that rain and fine weather should be positively connected with certain absolute heights of the barometer; its small motions, when it is more or less high, have the surest correspondence with the local weather; the fall indicating the presence of that subtle fluid which tends to decompose the air, and the ascent the cessation of this influence.

VI. If, during the decomposition of atmospheric air, the fluid operating this effect so unites with the ingredients of the electric fluid which had entered into the composition of that air, as to form a new compound in which the electric fluid does not possess its characteristic properties, rain only is produced, with little or no electric symptom: and this is the most common case. But when, from the nature of the new fluids which come to be spread in that stratum of the atmosphere, the decomposition of atmospheric air is such as to permit electric fluid to be produced by the precise ingredients (*i.e.* neither more nor less) necessary to its characteristic properties, it darts suddenly into the air in lightning: but this is only a first effect, and not yet thunder, a most astonishing phenomenon, consisting undoubtedly in successive detonations, such as the report of cannons fired in a rapid succession; and the former detonations must have with the latter this analogy of cause, that they are explosions of a particular expansible fluid, produced by that kind of sudden decomposition of atmospheric air, as happens by firing gunpowder and other processes.

VII. A direct proof of these sudden decompositions of some substances in such clouds, and simultaneous compositions of other substances, is the production of hail. This effect shows, that in a certain combination of circumstances, such a quantity of free fire enters suddenly into some combination, that the freezing point is much surpassed in the upper part of the clouds: hence the formation of grains of sleet so cold, that in falling through the clouds, their size is increased in the form of icicles, by the watery vesiculæ freezing upon them: of which formation the hail-stones bear all the characters, especially by having in the centre that opaque grain of sleet.

The foregoing are the most conspicuous of the operations produced, at certain times, in some strata of the atmosphere, but not all those which an attentive observer may perceive: they are here, as must be the case in the first steps concerning all invisible processes producing visible effects, explained only by general analogies with known causes in our chemical processes; and if we cannot yet approach nearer to specific causes, it is because we are still very backward in the knowledge of the subtle fluids, which, at different times,

come to mix with air in the atmosphere. We cannot, however, doubt that to such fluids is owing the multitude of phenomena still unexplained intelligibly, both in the atmosphere itself, and in its connexions with vegetation and the animal economy, when we consider what progress has been made in this knowledge, by only attending to the chemical affinities of light and fire, and by a beginning of discovery on those of electric fluid, the existence of which on bodies would be unknown to us, were it not for the motions produced in visible bodies by the disturbance of its equilibrium: this is one of its characteristic properties, and our test of the degrees of its intensity in different cases; as the thermometer is for free fire, and vision for light.

These first steps in the knowledge of causes, which are themselves imperceptible, must render experimental philosophers more and more attentive to all the circumstances that may lead to the discovery of new tests of the actual state of the air, in consequence of other impalpable fluids mixed with it; and also to the electric phenomena that may appear in chemical processes, since the electric fluid is always present on the bodies which enter into chemical combinations, as it is present on all bodies: in this diffused state, it produces no known chemical effect; but all the phenomena before pointed out undoubtedly prove, that its compositions and decompositions have the greatest influence in the terrestrial phenomena.

The general character of the system above, extracted from my works already published, by supposing a multitude of still unknown substances, will undoubtedly encounter the disapprobation of those philosophers who consider simplicity as the characteristic of the operations of nature: but if this word, has any sense, it must signify enough and nothing more; therefore, the first condition is enough, and when, in certain phenomena we find a deficiency of known agents, the chasm is not to be filled up by arbitrary hypotheses, which are nothing; analogy is our only sure guide in the investigation of hidden causes, as being a thread offered to us by nature itself.

This is one of the precepts of the father of true philosophy, the immortal BACON, who taught us, in particular, not to dread the multitude of substances when they are wanting for the production of phenomena accurately defined. Among his remarks on this subject is the following, under the 98th head of his *Silva Silvarum;* which remark I have the more admired the longer I have studied the phenomena of our globe:—"Cognitio humana determinata hactenus fuit speculatione et visu; ita ut, quicquid oculos fugeret, sive propter tenuitatem corporis, aut partes exiles, aut subtilitatem motus, parum sit exploratum. Hæc tamen naturam maxime regunt, illisque posthabitis, vera analysis institui nequit, aut indica rinaturæ processus. Spiritus aut pneumatica (expansible fluids) quæ omnibus tangibilibus insunt, vix cognoscuntur......Spiritus enim nihil sunt præter corpora naturalia, proportionaliter rarefacta, tangibilibus

corporum partis inclusa velut tegumento: neque minus inter se differunt, quam densa et tangibiles partes, omnibusque tangibilibus corporibus insunt plus minusve, et plerumque nunquam cessant. Ab his, eorumque motibus, præcipue procedunt arafactio, colliquatio, concoctio, maturatio, putrefactio, vivificatio, et præcipua naturæ effecta."*

Not to admit the existence of such substances, because they escape our sight or touch, would be returning back to occult properties, essential qualities, which, in the infancy of natural philosophy gratified the imagination under the shape of causes. However, these conceptions were a beginning of knowledge, as under that form were gathered a certain number of important phenomena, successively observed; but of these the agents were still to be sought for. However, it has been only at the birth of pneumatic physics, and when its progress has occasioned the investigation of the chemical affinities of light and fire, that many mysteries in nature have been unfolded; and what a field of new researches has been opened by the attention given to a third imponderable substance, the electric fluid! Now these very great steps teach us that no progress, marked by such memorable epochs, and followed by so many important consequences, can be expected, but by farther discoveries in the same class of substances, some of which may happen to manifest themselves also by characteristic effects, either known but mistaken, or yet unnoticed; and in these cases they might in some degree be submitted to analysis, by the changes they operate in certain phenomena, already known, but not sufficiently determined.

It is not to be expected, that, by groping in a desultory manner among the objects of nature, any main road of investigation can be opened for the discovery of new causes; as their effects are so much intermixed in perceptible phenomena, that we cannot ascend to them with certainty in a retrograde manner. Many more discoveries concerning them may be expected from researches carried on by connected steps along the roads already opened in the maze of imponderable substances, the greatest agents in the phenomena of nature.

The modifications of the sun's rays to produce heat, as followed by M. de Saussure and Dr. Herschel, and I may say by myself;

* "Human knowledge has hitherto been guided by viewing and beholding; so that whatever escapes our eyes, either from the smallness of the body itself, the tenuity of its particles, or the subtilty of its motions, is but little explored. By these, however, nature is chiefly governed; and if they be neglected, a just analysis cannot be made, or the process of nature disclosed. The expansible fluids that exist in all tangible substances, are scarcely known. These fluids are nothing but natural bodies, proportionally rarified, included in the parts of tangible substances as in a case: nor do they differ less from each other, than the dense and tangible parts, they inhere more or less in all tangible bodies, and for the most part are never still. To these, and their motions, are owing in particular rarefaction, dissolution, concoction, maturation, putrefaction, vivification, and the principal effects of nature."—C.

as well as the first observations made by Dr. Priestley on the chemical effects of light, have opened one of these roads, which requires to be pursued in all its ramifications. Much is to be done also concerning the nature of fire, *i.e.* the cause of heat, or of that expansion of bodies of which the thermometer is the measure; a road which has been much obstructed by the obscure idea of caloric, introduced in the modern theory of chemistry, at the time when several experimental philosophers were engaged in researches concerning the nature, modifications, and combinations of the expansible fluid long known under the name of fire. Much more remains to be done in the study of the electric fluid, its production and decomposition throughout so many phenomena. Lastly, almost every thing remains to be done to acquire some knowledge of a fluid, the existence of which is manifested by some characteristic effects, but which is itself totally unknown; though it cannot be without some, and it may be a great influence, in terrestrial phenomena: I mean the magnetic fluid, on which I shall say here only a few words.

Being now informed that the motions of bodies occasioned by amber when it has undergone friction, of which the cause was unknown to the ancients, are the effects of a fluid which has much greater functions in nature by its compositions and decompositions, when we come to consider the analogous, though much more limited effects produced by steel bars which have undergone proper frictions, we are led to conclude, not only that these particular motions are also the effects of a particular fluid, but that its functions in nature are not confined to those of attracting or repulsing iron according to circumstances, and producing in a moveable needle the property of keeping more or less parallel to the meridian of the place, with a determined end pointing towards the north: though the latter, by its use in navigation, is become of great importance.

With respect to this astonishing phenomenon, Prof. Van Swinden, of Franeker, has much advanced what Bacon calls the history in every class of phenomena, by an indefatigable perseverance in observing the variations of the magnetic needle compared with various circumstances. This, for every phenomenon, is the first step towards the discovery of causes; for the nature of those that may be devised must answer to all the modifications of the phenomena carefully observed, before confidence can be granted to any hypothesis.

In magnetism, the main point which must direct the natural philosopher in search of a cause is the same which directs the navigator, namely the direction of the magnetic needle; for this must belong to a cause, which in some manner influences the whole earth. This consideration has suggested to Prof. Prevost, of Geneva, an idea, which, though not completed, deserves notice. After all the discoveries already made in meteorology and chemistry, it cannot be doubted, that light has, in various ways, a great share in the formation of many atmospheric fluids, and thus probably of the magnetic: but there must be some cause of the formation of a greater quantity

of it on the northern than the southern hemisphere of the earth, since the magnetic needle has a tendency to turn that way. I shall not enter into particulars on M. Prevost's hypothesis, and shall only mention its ground, in order to show, that this object may not be unattainable; it is the circumstance, that the sun remains about eight days longer on the northern side of the ecliptic, than on the southern.

With respect to phenomena which may indicate a formation of this fluid, M. de Saussure has invented a very important instrument, which he has called a magnetometer; showing variations in the intensity of attraction of a magnet in different parts of the day, and also in different days and seasons, as the aërial electroscope shows variations in the electric state of the air in the same circumstances. These two kinds of variations, therefore, deserve to be followed, comparatively with each other, and in their connexion with other atmospheric phenomena, as these observations may forward our knowledge respecting the magnetic fluid, which probably, as well as the electric fluid, by its composition, decomposition, and combinations with other substances, has an influence in terrestrial phenomena.

The loadstone with respect to magnetism, and the tourmalin to electricity, are bodies which produce these phenomena from their own nature; but there is a method in our power to produce them by other bodies, namely friction: it is therefore very important in either case, to discover in what manner friction acts to produce these effects. We have yet no hold in this pursuit with respect to magnetic phenomena, but some light may be reflected upon them by a determination of the manner in which friction produces electric phenomena. I have studied this subject with much attention, and I propose to relate, in another paper, some experiments of this kind, leading to the analysis of the electric machine, and demonstrating the error of the idea of two kinds of electricities, or of two fluids acting in the electric phenomena.

Ashfield, near Honiton,
1st October, 1810.

Preparation of Perchloric Acid. By M. Nativelle.

(Extract from the Journal de Pharmacie.)

M. Nativelle first takes a general view of the usual method of preparing perchloric acid, and remarks, that "if the operation be closely observed, it will be found that the water added to the sulphuric acid is useless, or nearly so, and that the quantity of sulphuric acid employed is insufficient to accomplish the decomposition of the whole of the perchlorate of potash: because in this case, contrary to the ordinary law of substitution, the influence of mass, or an union of forces, if I may be allowed the expression, is required to overpower the pre-existing affinity; the perchloric acid, under certain

circumstances, being capable to displace the sulphuric. Having arrived at this inference, it only remained to ascertain the proportion of sulphuric acid necessary to decompose the whole of the perchlorate of potash. This proportion I will now state, which, although at first sight it may appear arbitrary, is nevertheless indispensable for obtaining a maximum of perchloric acid.

Place in a glass retort 500 parts of perchlorate of potash reduced to powder, and as free as possible from the chlorate. Add to this 1,000 parts of sulphuric acid at 66°, free from nitric acid: afterwards add 100 parts of distilled water. This small quantity of water is indispensable, otherwise the perchloric acid would be produced in a solid crystalized form. Fit to the retort a long adapter, terminating in a tubulated receiver, surrounded by cold water. Avoid luting the apparatus with paper, or other organic substance; for, when heated in contact with the vapour of perchloric acid, these substances would cause its decomposition, attended by feeble detonations. If the apparatus be well adjusted, lutes may be dispensed with; but if found necessary, asbestos will be most suitable.

By a careful application of heat, the whole of the perchlorate will soon dissolve; but attention will be necessary in regulating the heat, to prevent sulphuric acid from distilling over to the receiver, which is accomplished by keeping the contents of the retort below the boiling point. Perchloric acid rises in vapour at 316° Fah., a temperature much below that at which sulphuric acid distils. The end of the distillation may be known by the residue in the retort being transparent and colourless; or still better by the distilled drops falling slowly, even when the temperature is nearly sufficient to distil the sulphuric acid.

The quantity of perchloric acid obtained will vary accordingly with the manner of carrying on the operation, which will also regulate the quantity of sulphuric acid that is carried over. When the above described process is conducted with care, the 500 parts of perchlorate of potash will yield 300 parts of impure acid of about 45° density. If the distillation be pressed on too rapidly, the density of the impure acid will be nearly that of sulphuric acid, and the quantity equal to that of the perchlorate employed.

It is a very easy matter to purify this acid, by removing the sulphuric acid and the small quantity of chlorine which it holds in solution. To accomplish this separation, the impure acid is first to be agitated with a slight excess of saturated solution of sulphate of silver: the chlorine will be precipitated with the silver. This chloride of silver is to be separated by a filter and the acid placed in a large capsule, ready to recceive artificially prepared hydrated carbonate of baryta, which, being well washed, is to be added to the liquor, until the whole of the sulphuric acid has been precipitated, and a small quantity of perchlorate of baryta formed. This precipitate being also separated by the filter the liquor will now contain nothing but perchloric acid, combined with a small quantity

of perchlorates of silver and baryta. We next place it in a tubulated retort, and distil with the same apparatus, and with the same precautions as before. The liquor that first distils over being water only, it is to be received in a capsule, and the proper receiver not adapted till, by means of test paper, we are certain that the acid is coming over. In general, the more slowly the process is carried on the less water is retained in the retort, and the greater the density of the acid. Ice placed in the water surrounding the receiver is useful in condensing the acid vapour. The distillation may be carried on till the residue in the retort is nearly dry, taking care not to decompose the perchlorates of silver and baryta, which should remain behind. The perchloric acid thus obtained is quite pure, colourless, and transparent: its density varies from 60° to 65°; it is oleaginous like sulphuric acid. From 500 parts of pure perchlorate of potash, I have obtained 150 parts of concentrated perchloric acid.

In a future communication I shall describe the process for preparing pure perchloric acid in the crystalline form.

Mr. W. H. Balman's *Process for Obtaining Oxygen Gas.* Dated May, 1842.

A mixture of three parts of bichromate of potash and four parts of common sulphuric acid, placed in a capacious retort, and moderately heated, will yield pure oxygen with a rapidity entirely at the command of the operator.

This process is cheaper than that of heating chlorate of potash; for two parts of bichromate of potash, will produce as much oxygen gas as one of chlorate of potash, whilst the latter is nearly three times the price of the former: moreover, the residue of the first is valuable, and may be reconverted into bichromate of potash. It is also a more convenient process than any previously known, since it may be conducted at so low a temperature that a common retort and lamp will liberate a considerable quantity of oxygen.

On the Preparation of Cyanide of Potasssium. By M. Liebig.*

It is well known that one of the best processes of obtaining cyanide of potassium, is that of decomposing the ferrocyanide by heat. Amongst the several objections to this process, is that arising from a great loss of cyanogen, amounting to one third of the whole contained in the salt employed. This salt, which is composed of two atoms of cyanide of potassium, and one atom of cyanide of iron, suffers no change in its first named constituent by the action of the red heat employed in the process: but the latter constituent undergoes decomposition, which results in the formation of carburet of iron, and a liberation of nitrogen. The carburet of iron thus formed,

* *Annalen der Chime und Pharmacie.*

acting like a sponge, absorbs the fused cyanide of potassium; and in order to obtain any portion of it in a pure state, we are obliged to avail ourselves of solvents, generally alcohol. But as cyanide of potassium is endued with certain properties which, in chemical analysis are found to be extremely valuable, I have availed myself of these properties in order to simplify the mode of obtaining it.

If eight parts of ferro-cyanide of potassium be well dried by calcination on a hot iron plate; and afterwards finely powdered and mixed with three parts of dry carbonate of potash, and the mixture immediately thrown into a Hessian crucible, previously heated to dull redness, and the temperature maintained, the contents will first melt with a rapid disengagement of gas, and assume the appearance of a brown drossy mass. Shortly afterwards, as the mass becomes heated to redness, the dark colour begins to disappear: and by continuing the heat, the fused contents of the crucible assume a clear amber yellow. If, from time to time, a heated glass rod be introduced, and shortly afterwards withdrawn; the matter adhereing to it, when solidified, will, in the first instance, be brown; afterwards yellow; and at the end of the process, the fluid which adheres to the glass rod will be as clear and as colourless as water: and afterwards harden into a brilliant white crystalline mass.

During fusion, brown flakes, which will be observed floating on the fused mass, will ultimately unite into a clear grey spongy form. If the crucible be now removed from the fire, and allowed to cool a little, this grey matter generally sinks to the bottom: and more readily if the whole be agitated by the introduction of the glass rod. When the deposition is complete, the hot melted mass which floats above may easily be decanted into a warm porcelain capsule, without allowing any of the heavy powder to pass with it.

The mass thus separated from the iron will contain two distinct compounds: the cyanide of potassium, and the cyanate of potash; in the proportion of five atoms of the former to one atom of the latter.

During the fusion of the mixture of ferrocyanide of potassium with carbonate of potash, the following changes take place.

In an early part of the fusion, the cyanide of iron belonging to the ferrocyanide of potassium, and the potash belonging to the carbonate of potash, become decomposed, and their constituents form two other compounds: the cyanide of potassium, and the carbonate of protoxide of iron. At a higher temperature the former takes all the oxygen from the latter, and the results are, cyanite of potash and pure metallic iron.

Supposing the mixture to contain two atoms of ferrocyanide of potassium, and two atoms of carbonate of potash, it would be represented by the following formula:—

Ferrocyanide of Potassium.		Carbonate of Potash.		
$Cy_{12}\,Fe_2\,K_4$	$+$	$K_2\,O_2\,{}_2CO_2$	$=$	$Cy_{12}\,Fe_2\,K_6\,O_2\,{}_2CO_2$,

and after the fusion we would have,—

Cyanide of Potassium.		Cyanate of Potash.	Iron.	Carbonic Acid.
$Cy_{10}\,K_5$	$+$	$Cy_2\,O,\,KO$	Fe_2	${}_2CO_2$.

From two atoms of ferrocyanide of potassium, we obtain five atoms of cyanide of potassium: consequently, one-fourth more than by the fusion of that salt alone by a red heat. The small portion of cyanate of potash with which it is mixed will not interfere with any of its uses. The presence of the cyanate is easily detected by the liberation of carbonic acid, when the salt is saturated with an acid. An ammoniacal salt will also be found in the solution.

The explanation here given of the mode by which the cyanide of potassium becomes formed, is not quite satisfactory; because the carbonate of protoxide of iron is decomposed before the reduction of the iron, and the separation of carbonic acid into carbonic oxide and ferroso-ferric oxide takes place, and it is at the expense of these that an intermediate quantity (certainly not greater than indicated by the formula) of cyanate of potash is formed.

The sides of the crucible, and also the remaining metallic iron, are left covered with cyanide of potassium, which is recoverable by dissolution in hot water. Heat this solution, and dissolve in it a small quantity of sulphuret of iron; evaporate, and ferrocyanide of potassium is obtained. The mother-water will retain sulphuret of potassium.

NOTICES OF NEW PUBLICATIONS.

The Dictionary of the Arts, Sciences, and Manufactures: Illustrated with Eleven Hundred Engravings; by C. Francis, Esq., F.L.S.; *Author of the Analysis of British Ferns; The Little English Flora; The Grammar of Botany, &c.* W. Brittain, Paternoster Row.

(Second Notice.)

We expressed our high satisfaction of this work in a late number, and subsequent careful examination fully confirms our good opinion. We find upon calculation that more than 6,000 scientific and technical words have been collected, and of these a vast number are not to be found elsewhere. The labour of collecting them must have been immense. This alone renders the work truly valuable, nay indispensable, yet we think that a second design of the author, and one which he has admirably worked out, is even more valuable: he has given an accurate description of all useful apparatus and machines. There are more than 150 machines, &c., described on electricity alone, 13 thermometers, 21 different steam engines, and 14 presses; architecture, civil engineering, mathematics, mechanics, optics, and other departments of scientific knowledge are also liberally illustrated, the whole work containing, as we said on a previous occasion, no less than 1,100 engravings. Perhaps our readers may wish to judge for themselves of the character of the work from a portion of its pages, rather than trust to our opinion, we therefore give

the following extracts, chosen not because of any particular attention which Mr. Francis paid to the science of electricity, but because they are accordant with the ordinary subjects of which we are accustomed to treat; and although we have confined our attention to the words relating to electricity, yet by the references in italics the far greater extent of the subject will be implied.

"ELECTREPETER. An instrument for readily changing the direction of electrical currents. (See *Clarke, Bird, &c.*)

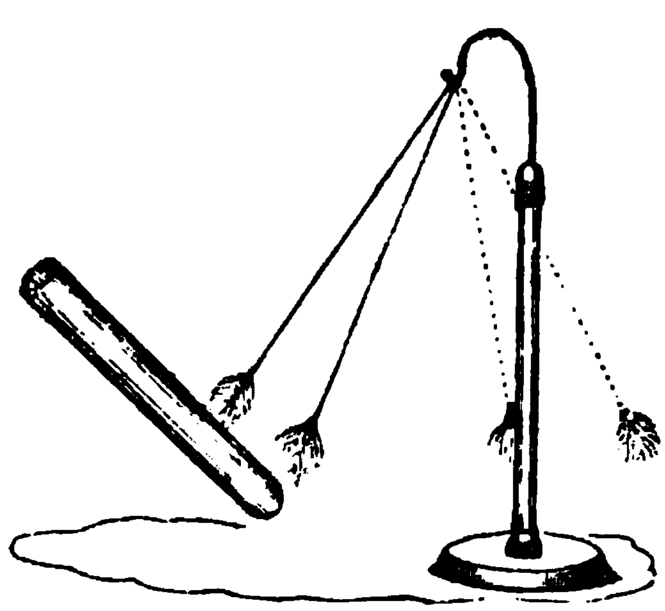
"ELECTRIC. All bodies are so called when they show electrical appearances upon being rubbed; or which will not permit the electric fluid to pass along them.

"ELECTRICAL, or ELECTRIC. Belonging to electricity.

"ELECTRICAL AMALGAM. (See *Amalgam.*)

"ELECTRICAL APPARATUS, consists of all the machinery used to illustrate the science of electricity; (for a particular description of each instrument see its substantive name, as *Dance, Condenser, Discharger, Leyden Jar, Magic Picture, Thunder House, &c.*)

"ELECTRIC ATTRACTION and REPULSION. One of the visible effects of a disturbance of the electrical fluid, when two bodies are excited or electrified in a similar manner, they repel each other; if excited so that one shall be in a different electrical state to the other, they will be attracted. Suspend two feathers on dry silken threads, and hold to them an excite glass tube, they will be attracted to it at first, being in a different state; but afterwards they will be repelled from it, and from each other, because they are then, through imbibing the fluid from the glass, both electrified similarly.



"ELECTRICAL CEMENT, for joining together the various parts of an electrical apparatus. Melt together five ounces of resin, one of bees'-wax, and one of red ochre. This may be employed in cementing the plates of metal in the galvanic troughs.

"ELECTRICAL CHARGE. The greatest quantity of the electric fluid which any body can be made to contain; or the greatest degree of disturbance which can be given to its inherent fluid. The restoration of that fluid to a state of quiescence, is called the discharge or shock.

"ELECTRICAL CIRCUIT. Any series of wires, or other conductors, intervening between the inside and outside of a charged Leyden phial, whereby the jar is discharged. Every thing which is to receive a shock must form a part of this circuit; and connecting the outside and inside of such a jar or battery, is called completing the circuit.

"ELECTRICAL CONFIGURATIONS. The peculiar radiating forms taken by certain powders when sifted upon charged electrics. They may be made as follows:—Put a sheet of dry glass on a piece of tin-foil spread on a table, and draw over the surface of it, in any fanciful device, the knob of a small charged Leyden jar; then, having ready tied up in a piece of loose muslin a mixture of equal parts of sulphur and red lead, in powder, sift them on the plate of glass, when it will be seen that the mixed powder will separate; and while the sulphur of it settles in a close line on the exact tract of the knob, the red lead will arrange itself at a little distance on each side, in the most beautiful star-like ramifications. The following gives but a faint idea of the effect:—

"ELECTRICAL CONDUCTORS. (See *Conductors*, and *Lightning*.)

"ELECTRICAL CURRENTS. The passage of the fluids from one place or object to another, through conducting substances: as for example, from the different sides of a charged jar; from one end of a galvanic battery to the other; along conducting wires, &c.; or when a current passes through a wire, which has another wire so near to it as to be affected by the passage of the fluid, the fluid which actually passes along the wire is called the *primary* current; and that effect or disturbance which takes place in the approximate wire is called the *secondary* current.

"ELECTRICAL EXCITATION. The power of disturbing the electric fluids, so that it becomes apparent either to our senses, or by the well known effects which it produces. Bodies thus influenced are said to be excited.

ELECTRICAL FLUID. That particular and universal power, substance, or property, which pervades all nature, occasioning, when disturbed, those appearances and effects, known in science as electrical and galvanic; besides numerous natural phenomena, such as lightning, the aurora borealis, and perhaps the whole of the chemi-

cal and magnetic action. Some philosophers maintain that all these effects are producd by different states of the same electric fluid; (see *Franklin.*) Others believe that two electric fluids exist; one of them always disturbed when the other is, and each acting in opposition to the other at all times. (See *Du Fay.*)

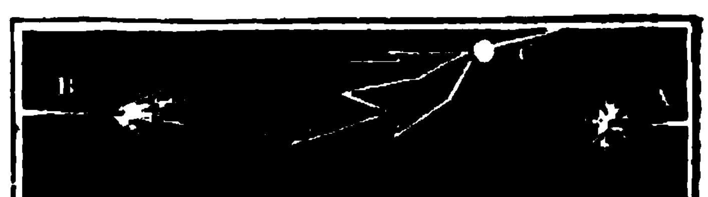
" ELECTRICAL INDUCTION. The power possessed by an excited body in influencing other bodies in the vicinity of it, without touching them.

" ELECTRICITY. A science which explains the laws which govern the excitation, distribution, and other phenomena of a peculiar element, called the electric fluid. Electricity, in its more limited acceptation, explains the electric effects produced upon various bodies by friction or pressure only. In its general meaning, it includes also, the explanation of those departments of science, called galvanism, electro-magnetism, and thermo-electricity. That effect, resulting from friction alone, constituting a branch only of a general subject, and which, for distinction sake, is called *common*, *free*, or *frictional* electricity.

" ELECTRICITY. *Disguised*, is when the electric fluid is accumulated upon the surface of a body, and yet has but little tendency to fly off, in consequence of that body being under the influence of another electrified body, which is near to it and insulated. The lower plate of the electrophorus is an example of disguised electricity.

" ELECTRICITY, *States of.* When the electric fluid in any body is so disturbed as to become apparent, one part of that body has the fluid in a redundant state; in the other part, it is deficient, or contains less than the quantity natural to it. The former, is called *accumulated* or *positive* electricity; and the latter, *negative*. Upon the supposition of two fluids, the former, is the *vitrious*; the latter, the *resinous* fluid. Free or common electricity is popularly divided into *atmospheric*, *animal*, *chemical*, *mechanical*, and *medical*, according to its effects, and the particular phenomena it explains.

" ELECTRIC LIGHT, *Brush*, *Star*, *Spark*. The spark or stream of brightness seen when a considerable quantity of the electric fluid passes through any imperfect conductor. If it pass into the air from a point electrified positively, it resembles a *brush*, as B. If from a negative point, a *star*, A. If it pass in a considerable quantity, and with rapidity, from one conductor to another, through the air, it will put on the appearance of a *spark*, more or less zigzag, C, and be attended by a snapping noise.



" ELECTRICAL MACHINE. Any instrument adapted to collect a considerable quantity of the electric fluid, as produced by friction. The principal electrical machines now in use are of two forms; in

one, a *cylinder* of glass is to be excited ; in the other, a *plate* of glass, (see *Cylinder* and *Plate.*) In either case, there are one or more cushions which rub against the glass ; and a prime conductor, (see *Conductor,*) to collect and retain the electric fluid given off by the glass.

"ELECTRICAL NON-CONDUCTORS. (see *Electrics.*)

"ELECTRIC POLES, OR ELECTRODES. The two opposite ends of a charged electric or galvanic apparatus. When the word pole is used, we distinguish them by the terms *positive* and *negative ;* but employing the term electrodes, the positive is called the anode or platinode ; the negative, the cathode or zincode.

"ELECTRIC SHOCK. The rapid passage of a quantity of the electric fluid through any substance, which occasioning a disruption of some bodies, and a convulsion to others, renders the term appropriate.

"ELECTRO-CHEMICAL ACTION. The chemical changes that take place owing to the interference or agency of an electric current.

"ELECTRO-CHEMICAL EQUIVALENTS, are the same, and coincide with the ordinary chemical equivalents.

"ELECTRO-CHEMISTRY. That division of electricity, which treats of the chemical effects produced by the passage of an electric current through a chemical compound.

"ELECTRODES. The poles of a galvanic battery. That pole in which the electric fluid enters, is called the negative pole, or electrode.

"ELECTRO-DYNAMIC CYLINDER. (See *Ampere.*)

"ELECTRO-DYNAMICS. That division of the science of electromagnetism, which explains the laws of all rotations, vibrations, and other motions occasioned by the mutual action of the magnetic and electric fluids.

"ELECTRO-GASOMETER. A small apparatus for collecting and measuring the amount of gas, resulting from the decomposition of water by electricity. (See *Bachhoffner, Clarke, &c.*)

"ELECTROLYTES. All substances susceptible of direct decomposition by a passage of the electric fluid through them, as water.

"ELEOTROLYTIC ACTION, or ELECTROLYSIS. Galvanic action considered in reference to chemical decomposition.

"ELECTRO-MAGNET. A bar of iron which assumes temporary magnetic properties, in consequence of a current of electricity being made to pass through it. It is made by twisting around the bar of iron, whether straight or shaped like a horse-shoe, a wire covered with silk or cotton, or other non-conducting material, and passing a current from one pole of a galvanic battery to the other, along the coil of wire. The circulation of the fluid through the wire, will render the bar of iron within it a temporary but powerful magnet, capable of sustaining a considerable weight. It loses this power the moment the connection with the battery is broken. In the cut, A is the electro-magnet ; B and C, the weight raised ; N and P, the wires from the poles of the battery.

"Electro-magnetic Apparatus. (See *Ampere, Callan, Bachhoffner, Clarke, Barlow, Sturgeon, Faraday, Marsh, &c.*)

"Electro-magnetic Coil. (See *Callan.*)

"Electro-magnetic Coil Machine. (See *Coil.*)

"Electro-magnetic Helix. (See *Helix.*)

"Electro-magnetic Machine, or Engine. A machine by which the effects of electro-magnetism may be noticed or ascertained.

"Electro-magnetic Multiplier. The original name of the galvanometer, an instrument for measuring the intensity of an electric current. The original instrument was merely a mariner's compass, with a covered wire coiled five or six times round it. When an electric current is made to pass along the wire, the compass-needle is driven out of its usual polar direction, the north end being turned east or west, according to the direction of the current. The following is another, but not a more powerful form of the instrument:—

A B are mercury cups, to hold the wires from the pole of the battery; C the magnetic needle, with a graduated card beneath it, and the coil of wire around it.

"Electromotive Force. Volta supposed that when two metals were in contact, a certain force was in operation, tending to effect a transfer of electricity from the one metal to the other. To this force he gave the name of electromotive.

"Electro-Magnetic Sphere. (See *Barlow* and *Sturgeon.*)

"Electro-Magnetism. The science which explains the action of the electric fluid and the magnet upon each other.

"Electrometer. An instrument to measure the quantity and quality of the electric fluid disturbed during any experiment or process. Some electrometers act upon the principle of electrical attraction or

repulsion, certain parts of them becoming divergent in proportion to the intensity of the disturbance of the fluid within them, such as Coulomb's electrometer, the quadrant electrometer, &c. Others depend for their action upon the circumstance that the electric fluid acquires momentum in proportion to its concentration, as in the balance electrometer, the medical electrometer, &c. (See *Balance*, *Coulomb*, *Lane*, *Medical*, *&c.*)

"ELECTRO-MICROMETER. Any instrument adapted to measure very minute quantities of electricity; synonymous with condenser.

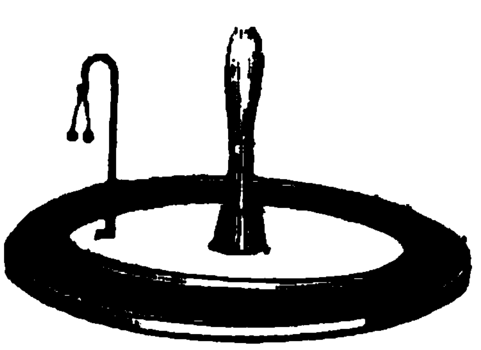
"ELECTRO-MOMENTUM. The power exerted by an electric current when suddenly turned out of its direct course; or, when made to pass from a good conductor to one which is less perfect.

"ELECTROMOTIVE. The power of motion conferred upon magnets, &c., by electrical action.

"ELECTRON, or ELECTRUM. The former is the Greek, the latter the Latin name for amber, which being rubbed, shows the property of attracting light substances; from which word and circumstance we derive the word electricity. Glass, and also gold, or an alloy like gold, was by the ancients likewise called electrum.

"ELECTRO-NEGATIVE and ELECTRO-POSITIVE. Those bodies which, when submitted to the action of a galvanic current, are apparently attracted to the anode, or positive pole of the battery, are called electro-positive, or cathions. Those attracted to the cathode, are electro-negative, or anïons; they being supposed to be in a contrary electrical state to the pole to which they are attracted.

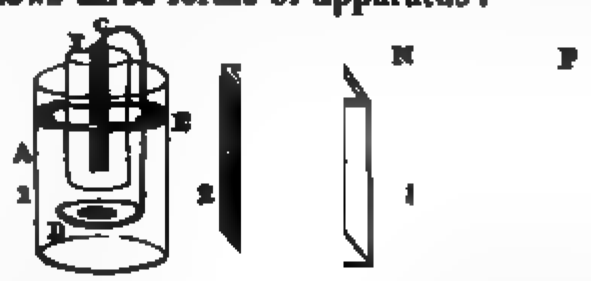
"ELECTROPHORUS. A simple instrument, which, when once excited, retains it electrical energy, which it is ready to give out continually for a long period. It consists of two plates; the lower one may be a plate of tin, ten or twelve inches in diameter, with the edges turned up, so as to hold the following composition when poured hot into it, forming a cake when cold, of about one-eight, or from that to a quarter of an inch thick. The composition is, pitch 1 part; asphaltum 4 parts; and bees'-wax 1 part; or pitch, rosin, and bees'-wax in the above proportions. The upper plate may be of wood, covered completely with tin foil, and having a glass handle to lift it by. It is two or three inches less in diameter than the lower stand. When to be used, the instrument is to be warmed, the resinous plate rubbed with a piece of warm flannel, and the upper plate put upon it, its glass handle being previously dried. Lifting up the upper plate by the handle the edge of it will give a spark; touch the upper plate with the finger, and put it down again on the other; upon lifting it a second time, it will give another spark, and so on for a considerable period. The upper plate has often a wire with two pith balls attached to it.



"ELECTRO-PULSATIONS. Electric currents or shocks, which pass in such rapid succession between the two sides of a charged Leyden jar, or between the poles of a galvanic battery, that the shocks are not to be individually distinguished.

"ELECTROSCOPE. Any instrument to indicate the disturbance of the electric fluids; but not of sufficient accuracy to show the precise amount of that disturbance, such as Bennet's gold-leaf electroscope and Saussure's pith ball ditto.

"ELECTROTYPE. A method of taking reverse fac-similes of medals, coins, copper-plates, seals, &c., by means of the power which voltaic electricity has of decomposing metallic salts. A piece of zinc is soldered at one end of a wire, and the medal, seal, &c. (if a seal, or other non-metallic body, it must be previously covered with black-lead), at the other end, and then the medal immersed in a saturated solution of copper, and the zinc end in acidulated water, there being some membrane or other porous substance between the two solutions. This being altogether a galvanic circle, decomposition of the sulphate of copper will take place, and metallic copper be deposited on the medal; after some hours the deposit will be thick enough to remove, and will be found an exact reverse impression of the medal. The following cut shows three forms of apparatus:—



In No. 1, A is a jelly pot. F a porous tube within it. C is the wire, with a bar of zinc inside the tube F, and the lower end bent, holding the medal D upon it. E is a shelf surrounding the tube F, to hold crystals of sulphate of copper. No. 2 is a square wooden box, with a division of plaster of Paris across it. No. 3 is a glass jar, holding a copper-plate to be copied, and also another copper-plate; when these are connected with the poles of a galvanic battery, one of the copper-plates will be dissolved and deposited on the other."

So also under magnetism, galvanism, thermo-electricity, the list of words are equally extensive, and their meanings well defined. In short, the industry and talent which is every where shown in this cheap and valuable volume, merits the strongest encouragement. It sets the example of simplicity combined with scientific accuracy, and shows equally the unassuming character and yet extensive reading and profound knowledge of the author.

The Ninth Annual Report of the Royal Cornwall Polytechnic Society. London: Simpkin and Marshall, Stationers' Hall Court; and Weale, 59, High Holborn.

THE Royal Cornwall Polytechnic Society, whose annual reports we have regularly noticed for several years past, is still progressing

in usefulness, and extending its sphere of action. The present report exhibits a pleasing and animating picture of the advances which this, the most useful of British scientific institutions at this day in operation, is rapidly making, in cultivating the minds, in discovering the genius, in encouraging the invention, in stimulating the industry, and, consequently, in improving the general character and happiness of thousands of our fellow men, inhabitants of its extensive and peculiar locality: as well as in contributing essentially to the general weal of the British realms.

We shall present our readers with a general outline of the contents of the "Report" in a future early number.

On a simple and cheap Method of preparing Hydrochloric Acid: absolutely pure, and of any required strength. By W. Gregory, M.D., Professor of Chemistry, King's College, Aberdeen.

Much difficulty is experienced in procuring pure and concentrated hydrochloric acid for chemical purposes: the acid of commerce containing various impurities, particularly sulphuric and sulphurous acids, free chlorine, chloride of iron, and sulphate of soda: these arise from the impurities contained in the materials employed in its manufacture. The chlorine from the action of nitric, or nitrous acid, (often present in oil of vitriol) or the hydrochloric acid: sulphurous acid, from organic particles in the common salt employed: and chloride of iron from the presence of that metal also in the salt. Pure and clean materials are therefore the first requisites for obtaining a pure acid. Dr. Gregory finds that, if to one equivalent of salt, *two* equivalents of sulphuric acid, diluted with a certain quantity of water be used, instead of *one* equivalent, as usually prescribed, the whole of the hydrochloric acid may be expelled without a trace of sulphuric acid passing over, even to the first condensing bottle: and that two-thirds of the hydrochloric acid distil over before water is volatilized. On this observation the following process is founded.

Into a common Florence flask are to be introduced four ounces of the purest patent salt, and five fluid ounces of sulphuric acid of specific gravity 1·600; a gentle heat is to be applied and the gas which is thus generated, is to be conducted, by a bent glass tube, into a four-ounce phial, containing two ounces of distilled water, and surrounded with snow, or ice-cold water. No safety tube will be required, as the tube is to be only about one-eighth of an inch below the surface of the water; so that should any absorption take place, the rise of a little water in the tube will expose the extremity of it, so as to admit air; or, for greater security, a small ball may be blown on the descending branch of the tube.

The gas will be absorbed as fast as it comes over, and for the first hour and quarter the heat will hardly require increasing. If the temperature of the surrounding water be kept as low as 50°, the

two ounces of distilled water will increase in volume to about three ounces of colourless hydrochloric acid, foaming strongly, and have a specific gravity of 1·20 to 1·21, the gas passing over so dry that no part of the tube will become warm. The first portion being removed, its place is to be supplied by two ounces more of distilled water, and the heat gradually increased for an hour longer: at the expiration of which, all the hydrochloric acid, with some water, will be expelled, and the two ounces of water have become three ounces of hydrochloric acid, of specific gravity 1·10. Both portions are absolutely pure. If three ounces of water be used in the first instance, four and a half fluid ounces of acid, specific gravity 1·165 would be obtained: and then replacing this acid by two ounces of distilled water, *three and a half* ounces more acid, of specific gravity 1.065, would be obtained. Should five ounces of water be used for the first condensation of the fumes, and continued till the distillation be complete, *seven and a half* fluid ounces of acid, of specific gravity 1.155 would be the result.—*Proceedings of Chemical Society.*

Preparation of Chloride of Zinc. By M. Righieri.*

Take of pure crystallised chloride of barium 80 parts: pure sulphate of zinc 98·6 parts; and distilled water 1,500 parts. Divide the water into two equal parts, and in the one dissolve the chloride of barium, and in the other the sulphate of zinc. Mix the two solutions in a mattrass, and hasten the decomposition by the heat of a sand-bath, applied for a few minutes. Filter the liquid and evaporate on the sand-bath until about 60 parts only of the liquor remain; which must be passed through a fresh filter on which is placed some animal charcoal mixed with a little powdered chloride of barium. After this last filtration the chloride is to be again evaporated, until the formation of brilliant white crystalline flocks; which, when dry, are to be preserved in a well closed vessel.

Atomic Weight of Zinc.—By converting zinc into an oxide through the agency of either the nitric or the sulphuric acid, and afterwards recovering the metal by heat, M. Jacquelain has been enabled to determine the weight of the oxide of zinc, and appoint the atomic weight of the metal; which, according to his account, is 414; which is greater than previously supposed.

M. Jacquelain has analysed a specimen of commercial zinc, and has found in 100 parts, 99·19 of pure metal, the other being carbon, iron, and lead, the last being the principal adulteration.—*Comptes Rendus.*

* *Journal de Chimie Médicale*, 1842.

THE ANNALS

OF

ELECTRICITY, MAGNETISM,

AND

CHEMISTRY;

AND

GUARDIAN OF EXPERIMENTAL SCIENCE.

OCTOBER, 1842.

Memoir on Magnetism.[*] *By* M. F. Savary.

(Read at the Academy of Sciences, July 31st, 1826.)

Previous to the exposition of the phenomena which form the subject of this memoir, I believe it is my duty to recall the recent discoveries that have served me as a starting point, and some of the experiments to which they have given birth.

It is only a few years ago when no other cause of magnetization was known than the influence of bodies already magnetized, and the magnetic force of the terrestrial globe. To this latter action was attributed by many, not only the effects of the shock, but of the thunderbolt, and the accidental magnetism without constant direction observed by Franklin, Van Marum, Priestley, and other philosophers, in the fragments of iron wire traversed and broken by a strong electrical discharge.

In 1820, a short time after the discovery of Œrsted, M. Arago observed that iron filings are attracted and sustained by the wire which joins the poles of an electro-moving apparatus; that they are detached the instant that the communication is interrupted;[†] that the current gives to steel needles a permanent magnetism, in a determined direction, perpendicular to its own proper direction, independent of the magnetic action exercised by the earth; and that two needles, parallel to each other, forming with the conducting wire a right angle, placed at equal distances on each side of this wire, acquire the same degree of magnetism, but in opposite directions.

M. Ampere, who since that time represented a magnet by a system of closed currents, perpendicular to the magnetic axis, proposed to

[*] *Annales de Chimie et de Physique*, tome 34.

[†] M. Arago has sometimes found that the filings became attached even to a platinum wire, some instants after the communication had been broken.

roll the wire of the conductor as a helix. The result was such as he had expected, as to the energetic action of these helices to magnetize parallel to their axis a needle enveloped in their spirals.

M. Arago recognized that in the interior of a helix, sufficiently long in relation to its diameter, and of a very short interior diameter, needles parallel to its axis, but distributed in any manner whatever, acquired magnetic intensities sensibly equal. At the exterior the same helix has very little magnetizing power, still less when it is longer and the spirals are nearer to each other. It is easy to represent these results by means of the analytic law proposed by M. Ampere for the mutual action of two elements of the currents.

In these experiments, voltaic currents only were employed. M. Arago soon made known a species of analogous action, but differing in many respects. He found that currents of ordinary electricity, produced by friction-machines, possess the property of rendering steel magnetic, as soon as on interrupting the conductor the fluid was forced to flow off in a series of small sparks; that a continuous current, when it flows off into the earth by a long metallic wire, produces no magnetism in steel and exercises no attraction on iron filings.

M. Rodolfi, a long time after, announced that on making a communication between the cushions and the conductor of an excellent electrical machine by means of a helical wire, he succeeded in magnetizing steel needles by the action of the continuous current *(tacita corrente)* that traversed the wire.

By electrical discharges, as also by the pile, the direction in which magnetism is developed is perpendicular to the direction of the conducting wire.

A useful application of this important property, which he had discovered afterwards, presented itself to M. Arago. Magnetism furnished him with a very simple and exact means of comparing the conductibility of different bodies, for the electric fluid accumulated up to high tensions and on large surfaces. This method not having yet been published, he has readily permitted me to give a description of it here.

Let us suppose a conducting wire setting out from the exterior armature of a battery, that it be rectilinear in one portion of its extent, that it then becomes ramified in a certain number of branches of the same metal, all of equal diameter, form, and length, and that they re-unite in one common point. We then place transversely on the straight part of each wire, before and after the ramification, some steel needles, and then pass a discharge through the whole system. It will traverse the first conductor entirely, and become divided amongst the different branches in equal portions. The magnetism of the needles placed on the first wire will then be the measure of the effect produced by the total quantity of electricity: the magnetism of the needles placed on the ramifying wires, the measure of the effect by a certain fraction of that quantity; the third, if there are three branches; the tenth, if there are ten. We shall thus form

a scale of magnetic intensities, in relation to any fraction whatever of a given discharge. If afterwards, substituting at the different branches, all of the same metal, similar wires of different metals, we pass a discharge through this new system, a second discharge equal to that the action of which is known, it will become unequally divided among the different wires, and similar needles placed transversely on each of them will indicate by the degree of their magnetism, whether one metal has transmitted the third, another the fourth, or another the tenth of the total quantity of electricity.

Since the researches of M. Arago, M. Nobili has published some interesting experiments on magnetism. One of them consists in making either an electrical discharge or a current from a pile (M. Nobili has never separated the two methods of magnetizing) traverse a spiral plane of copper wire. If between any of the isolated spirals we fit perpendicularly to their plane some steel needles, we find that the needles situated near the centre, and the needles near the circumference, are magnetized in a contrary direction; and that consequently at a certain distance from the centre the magnetism is neutral. It was easy to foresee this result, since after the long and well known action of connecting wires, the spirals comprised between the centre and a needle exercise on it a contrary action to the exterior spirals.

I have repeated the experiment of M. Nobili in order to obtain some series of exact measures, and I have found that the distance from the centre at which there is no magnetism produced, at least by a feeble pile, may be calculated with sufficient precision by the formula

$$\int \frac{a\,d\,\omega}{R} - \iint \frac{d\,\omega\,dt}{r} = 0,$$

t represents the vector ray, ω the angle from the centre for any point whotever of the spiral,* r, the distance of the extremities of the needle at this point, a and R the same quantities relative to the exterior circumference, by which we may suppose the spiral to be terminated. This formula is founded on the analytic law proposed by M. Ampere.

And then, as to the magnetic actions of other bodies besides iron, until the brilliant discovery of M. Arago, we only knew of the experiments of Coulomb, who himself regarded the consequences as doubtful; and the traces of magnetism observed by M. Ampere in copper wires submitted to the action of a very powerful voltaic current, or by M. Becquerel in small needles of different substances, enveloped in the spirals of an energetic multiplier. I pass now to the exposition of the researches to which I have devoted myself, on the different means of magnetizing already

* Or rather the ray from a circumference of the circle passing by this point with which the entire spiral is sensibly confounded, the limits are

$$\omega = o\ ;\ t = o$$
$$\omega = \pi \quad t = a$$

referred to, in which researches I have had the advantage, which will be easily perceived, of being sustained by the kindness and the counsel of M. Arago.

I shall occupy myself at first on the magetizing action of electrical discharges transmitted, first by rectilinear conducting wires, second by helical wires; I shall afterwards explain the influence which the other metals besides iron and steel undergo and exercise in this phenomena; and lastly, I shall speak of analogous actions, produced by the continuous currents of the pile of Volta, and by magnets themselves.

On the Action of Electrical Discharges Transmitted by Rectilinear Conducting Wires.

The following experiments having for their object the making known of the general phenomena produced by very energetic actions, I was obliged to employ very fine needles. They were disposed in horizontal directions at different heights perpendicularly to the wire, which was extended in a right and horizontal line, their middle corresponding vertically to this wire. For this end it is advantageous to fix them along the edge of a plane very slightly inclined, in such a manner that the first being in contact with the conductor the others are a little removed from it but almost insensibly, and are still found to be sufficiently spread to prevent their mutual influence having any appreciable power.

I now proceed to transcribe the result of some experiments; each of which was repeated several times, and all agreed in a satisfying manner.

I took a platinum wire of $\frac{1}{4}$ of a millimetre in thickness, and about two inches long. It was extended in a right line at a distance of more than a centimetre from the ruler which served to support it. The needles, tempered hard, were $\frac{1}{4}$ of a millimetre in diameter, and 15 millimetres in length. After having magnetized them by the discharge of a battery of twenty-two feet surface, which was still far from being charged to saturation,* they were made to oscillate. I shall indicate the direction of the magnetism developed in each needle by calling those needles positively magnetized whose poles are placed in the direction in which the current of a pile traversing the conducting wire would have deflected them

* The indications of the electrometer are not given here because they could not be made use of to show whether two discharges slightly differed from each other. I have since employed, as an exact measure of tension, the balance of Coulomb, with this alteration, that the needle carrying the *moveable* disc, instead of being glass, is metallic; the fixed ball, instead of being insulated, is supported by a thin stem of metal: then, by means of the silver wire to which the moveable needle is suspended, we establish an exterior communication between the moveable disc and the fixed ball. If afterwards we make the whole system communicate with the interior armature of a battery, the distances to which the disc is repulsed indicate at each instant the state of the electrized surface. This apparatus thus modified possesses all the desirable precision.

if they had had a previous magnetism, and which if suspended above this wire, would have no power to move except in horizontal planes. This direction is the same whatever be the distance of the needle from the wire. We must understand by the words, *needles negatively magnetized*, those whose poles will be deflected in a contrary direction. This admitted, I now give the times of the observed oscillations :

	Mill. of wire	Duration of 60 oscillations. m.	s.		Direction of the magnetism.
1st needle in contact with the wire during the discharge ..*		0	52	4	Positive.
2 during the discharge had ..	1 2	1	3	8	id.
3	2 5	1	12	8	Negative.
4	3 7	0	44	6	id.
5	5 0	0	40	0	id.
6	6 0	0	41	8	id.
7	7 4	0	44	8	id.
8	8 5	0	58	2	id.
9	9 7	1	20	1	id.
10	10 9	1	52	0	Positive.
11	11 8	1	18	6	id.
12	12 5	1	1	0	id.
13	13 8	0	49	6	id.
14	16 3	0	38	2	id.
15	18 7	0	33	8	id.
16	21 0	0	31	3	id.
17	23 8	0	29	5	id.
18	28 5	0	30	8	id.
19	34 0	0	29	8	id.
20	46 0	0	35	9	id.
21	70 0	0	55	6	id.
22	100 0	1	27	6	id.
23	130 0	1	48	0	id.

We see that the direction of the magnetism produced by the discharge has changed twice; the first at about two millimetres of wire; the second at nearly 10 $^{\text{mill.}}$ 4. The *maximum* for the needles negatively magnetized, is found at 5 $^{\text{mill.}}$ of wire. The *maximum* of the positive needles are, the one in contact with the wire, the other at about 3 $^{\text{cents.}}$ in height. At this latter *maximum* the needle made 60 oscillations in 29″, 7. On being magnetized to saturation by strong magnets, it made sixty oscillations in about 23″. No dis-

* The needles during the discharge, were about 2 cent. from each other; and as the latter ones would have been too near the extremities of the wire if they had only a single support slightly inclined, they were disposed on several parallel planes between them, and invariably connected.

charge within the limits I had in my power of employing, produced this state of saturation, the conducting wire remaining the same and of the same length.

The needles as strongly magnetized in one direction as in the other did not present either consequent points nor multiple centres. This may easily be proved by examining the curves formed around them by iron filings.*

By submitting to the same discharge needles of the same degree of hardness and of the same diameter, but of very different lengths, 15$^{mill.}$, 10$^{mill.}$, and of 5$^{mill.}$, we find that the distances from the wire at which the changes in the sign takes place, do not differ from two or three-tenths of a millimetre; that is to say, quantities for which we can scarcely answer in the first experiments.

As a second example I took a wire of half the length (1 metre), of the same platinum wire. For a similar discharge to the preceding one the form of the series of magnetic intensities is thus, as we shall see, entirely changed.

Needles of 15 mill. in length.	Distance of wire during the discharge.		Duration of 60 oscillations.			Direction of the magnetism.
	Mill.		m.	s.		
†1st needle in contact with the wire	0	0	0	3	1	Positive.
2	1	1	2	29	0	Negative.
In another series at	1	4	1	48	3	id.
3	2	0	Without appreciable magnetism.			
4	3	0	1	25	6	Positive.
5	4	3	1	5	6	id.
6	5	5	1	3	0	id.
7	6	7	1	13	6	id.
8	8	0	1	32	2	id.
9	8	6	3	8	0	Very slightly negative.
10	9	6	1	34	8	Negative.
11	10	5	1	17	2	id.
12	12	3	1	1	2	id.
13	13	5	0	56	4	id.
14	14	6	0	56	0	id.

* I have endeavoured since the reading of this memoir to determine the position of the poles by sliding small needles, suspended horizontally, along a very fine copper wire, vertical, and traversed by a voltaic current. We know that at the poles themselves the action of the wire for making the needles turn is null, and that on both sides of these points it is exercised in a contrary direction. We find also that for very different intensities they are always strong near the extremities, and suddenly approach the centre, when the magnetism is on the point of changing its sign, as if the middle needles alone preserved, at that time, any magnetism. We may conceive by that how the direction in which these needles are magnetized by the discharge hardly depends on their length. It yet remains for me to submit them to a later species of proof to which I have submitted the needles magnetized in helices, that is to study the magnetism of their fragments after having broken them.

† This needle presented at its extremities traces of multiple centres.

Needles of 15 mill. in length.	Distance of wire during the discharge.	Duration of 60 oscillations.		Direction of the magnetism.
	Mill.	m.	s.	
15th needle in contact with the wire	15 7	0	59 4	Negative.
16	16 9	1	3 0	id.
17	18 2	1	5 0	id.
18	19 1	1	16 8	id.
19	20 0	1	34 2	id.
20	20 9	2	29 0	id.
21	21 4	Almost null		Slightly negative.
22	23 3	1	23 7	Positive.
23	32 7	0	41 4	id.
24	44 0	0	34 0	id.
25	70 0	0	43 2	id.
26	100 0	1	2 2	id.
27	130 0	1	28 2	id.

This series presents four changes in the direction of the magnetism. The first takes place at less than a millimetre from the wire; the second at 2$^{\text{mill.}}$; the third at 8$^{\text{mill.}}$, 4; the last at 21$^{\text{mill.}}$, 5. We see that needles which were negative in the first series are positive in this, and *vice versa*. The latter *maximum*, which is found at the height of 3$^{\text{cent.}}$, is 4½$^{\text{cent.}}$. Its value is less; it being only 34″.

In the same circumstances, for needles ten millimetres long, the same changes of signs take place at 0$^{\text{mill.}}$, 5; 2$^{\text{mill.}}$, 5; 9$^{\text{mill.}}$, 5; and about 20$^{\text{mill.}}$, 5. For needles of five millimetres in length, at 0$^{\text{mill.}}$, 5; about 3$^{\text{mill.}}$, 2; 10$^{\text{mill.}}$, 5; and about twenty millimetres. These distances are almost the same as were obtained with needles of fifteen millimetres.

I now proceed to a third example. For the same distance charge a platinum wire, rather thicker than the preceding one, of 0$^{\text{mill.}}$, 37 diameter, and of the same length (one metre), gave changes in the sign four times, nearly at the distances of three, five, nine, and twelve millimetres from the wire. From thence up to 2$^{\text{cent.}}$ we only find the needles very feebly magnetised; between which, with stronger discharges, a third period was manifested, except at the changes of the signs, at least by the variations of the intensity, just as I have observed it in a series obtained with a brass wire of an equal sensible diameter. For the platinum wire of which I have just spoken, the needle the most magnetized was that found at a distance of about 6$^{\text{cent.}}$ from the wire. It took 56″ to make sixty oscillations. The needle possessing the most magnetism for a finer wire, and of the same length as before specified, made the same number of oscillations in 34″, and was at a distance of 4½$^{\text{cent.}}$ from the wire.*

* On the same platinum wire of 0$^{\text{mill.}}$, 57 diameter, but 0$^{\text{met.}}$, 65 lengths only, and for a discharge stronger than the preceding ones, small needles of five millimetres have changed the direction of their magnetism five times;

In order to finish the comparison between different platinum wires I chose one much finer than the preceding ones. It was ⅛ of a millimeter in thickness. A meter in length of this wire, with a discharge equal to the preceding one, gave the following results :—

Needles of 15 mill. in length.	Distance from the wire during the discharge.		Duration of 60 oscillations.			Direction of the magnetism.
	Mill.		m.	s.		
1st in contact with the wire......	0	0	0	34	4	Positive.
2	1	2	0	25	2	id.
3	2	6	0	24	1	id.
4	5	0	0	24	3	id.
5	7	4	0	24	0	id.
6	9	7	0	23	3	id.
7	12	0	0	23	5	id.
8	15	1	0	24	9	id.
9	18	5	0	27	0	id.
10	22	5	0	25	8	id.
11	28	0	0	27	0	id.
12	34	0	0	30	9	id.
13	45	0	0	37	6	id.
14	70	0	1	1	8	id.
15	100	0	1	37	2	id.
16	130	0	2	0	5	id.

This series presents no changes in the sign. At the commencement the variations of intensity are scarcely observable. It is important in this respect: 1st, the *maximum* of magnetic intensity is found at the height of 11 [mill.] about five times nearer the wire than when we employed a platinum wire three times as thick; 2ndly, this *maximum* is the state of saturation which we obtain by the magnets, and its value is nearly six times greater than the value given by a wire of triple its diameter. At the same time that the *maximum* approaches the wire the decrease of the intensities of this *maximum* from thence becomes more rapid.

If we pass through a wire $\frac{1}{8}$ of a millimetre in thickness and one metre in length, a series of discharges, diminishing their strength by degrees, we should see the *maximum* very slowly diminish as it approached the conductor; but we never obtain changes in the sign of the magnetism, and never any analogous series to those presented by the other wires. On the other hand, by gradually diminishing the discharges, we never obtain with the other thicker wires, and of the same length, the series just presented to us by the finest wire. The changes of sign quite disappear then, and we only observe variation of intensity in their place, but at the same time the value of *máxima* diminish in proportion as they are found at less heights.

the last change took place at twenty-eight millimetres from the wire. The needles in contact with the wire were even magnetized negatively, and the most magnetized of all. It was necessary to remove to a distance of 10 [cent.] from the wire to obtain a slight degree of magnetism.

If, on the contrary, we cause to traverse these wires of a still greater diameter, without changing their lengths, discharges of a stronger character than those whose effects have been described in the preceding paragraph, we shall see the *maximum* of magnetism augment a little in value; but this is by increasing the distance more and more, and the series changes in form, but without coming into any of the other series, at least in the limits of the electric force of the battery employed.

In the preceding experiments a given discharge has always produced a magnetism as much stronger as the length of the wire was greater in proportion to its diameter. This increase has a limit to its effect. The following series gives for a similar discharge, but much more feeble than the preceding ones, their lengths of the same platinum wire of $\frac{1}{8}$ of millimetre in diameter.

Length of the wire 0 met. 50.

Needles of 16 mill. in length.	Distance from the wire during the discharge. Mill.	Duration of 60 oscillations. m. s.	Direction of the magnetism.
1	0 4	1 0 0	Positive.
2	1 3	1 57 5	Negative.
3	2 5	0 54 0	id.
4	5 0	1 21 6	id.
5	6 1	1 28 8	Positive.
6	7 6	0 47 2	id.
7	9 0	0 37 9	id.
8	11 4	0 31 7	id.
9	13 8	0 29 4	id.
10	21 0	0 29 8	id.
11	33 0	0 34 2	id.
12	44 0	0 45 6	id.

A needle placed at a considerable distance from the third, and apparently at the same distance from the conducting wire, was equally negative, and made 60 oscillations in 54″ 8.

Length of the same wire 1 met. 0, and 4 met. 30.

Needles 15 mill. long.	Distance from the wire during the discharge. Mill.	Duration of 60 oscillations. Length of wire. 1 met. m. s.	Duration of 60 oscillations. Length of wire. 4 met. 30. m. s.	Direction of the magnetism.
1	0 0	1 6 4	0 39 8	Positive.
2	1 2	0 31 6	0 29 8	do.
3	5 0	0 25 5	0 26 1	do.
4	8 5	0 25 5	0 29 9	do.
5	11 4	0 24 6	0 36 7	do.
6	22 6	0 31 4	1 12 2	do.
7	34 0	0 44 0	1 58 5	do.
8	45 0	1 6 2	2 26 5	do.

We see in comparing the three preceding series, 1st, that the wire of a metre in length gave absolutely the most elevated *maximum*; 2ndly, that the relative *maxima* in each series were found nearer the wire in proportion as this wire was increased in length, the diameter still remaining the same.

In general, with the same battery, and with wires of the same nature, and with similar needles, the form of the series depends on three things: the intensity of the discharge, the diameter, and length of the wire. The discharge and the length of the wire being made constant, there is a diameter at which the *maximum* of magnetism has the highest value; for diameters greater or smaller than this the *maximum* is less. The discharge and the diameter being kept the same, we find a length at which the *maximum* is greater than for any other length either greater or smaller.

The length and the diameter which thus give the greatest degree of magnetism, is the one which is the smallest, and the other that which is the greatest, in proportion as the wire has less conducting properties.

The *maximum* of magnetism is further from the wire, the number of changes in the sign in the interval much more considerable, as the length of the wire is less in proportion to its diameter.

We obtain similar series by means of equal discharges, by varying at once the length and the diameter of the wire in a certain proportion.* When the length and the diameter of two wires are not satisfactory on this point, we can not in general, by means of discharges, either equal or different, obtain from the two wires exactly parallel series. The disparity is especially manifested when the wires and the discharge are in the necessary conditions for producing change in the sign of the magnetism with a change of distance.

I have not yet made a complete series with wires whose thickness was less than a millimetre. But I am convinced that a brass stem of five millimetres in thickness, will give needles magnetized in different directions according to the intensity of the discharge.

Hence that part of the conducting wire extended in a right line to a sufficient length, and the form of that part which is not so extended, exerts no influence on the magnetism, whilst its length does so to a considerable extent, as we have just seen.

All the points of the wire exercise equal actions, at least in these inconsiderable lengths. In fact, at the same height, those needles which were at a great distance from each other, providing that they were not too near the extremities of the rectilinear part, received exactly the same quantity of magnetism, and in the same direction. This is true again when amongst the needles, some of them are

* This proportion differs sensibly from the simple proportion which, according to the researches of M. Davy and M. Becquerel, ought to exist between two conducting wires of the same nature, because they transmit with an equal intensity a voltaic current of equal strength. We know then that the relation of these wires to their transverse sections ought to be constant.

enveloped in glass tubes, sealed with gum lac, and the others exposed without envelope to the action of the current. This latter proof is only a repetition of one of the first experiments of M. Arago. It does not permit me to suppose that a part of the discharge, when the wire is very fine, is transmitted by the air and the neighbouring bodies, or at least that such a part could have any sensible influence. In fine, in order to discard the supposition that the strips of dry wood placed under the wires could exert any influence, it will suffice to dispose the needles some above and some below the horizontal conductor, in such a manner that the latter are very near the plane which serves for a support. We find, as we might easily foresee, at equal distances from all parts of the wires that there is the same magnetic intensity and contrary polarities.

In order to study the mutual influence of the different parts of a circuit, I disposed three series of needles of three brass wires, placed one after the other, and joined together at their extremities. Each wire was a metre in length: the first, situated between the two others, $0^{mil.}$, 125, the second $0^{mil.}$, 375, the third $0^{mil.}$, 75, in diameter. The first needle had a *maximum* of positive intensity, which gave 60 oscillations in 36″; for the finest and farthest insulated wire, the maximum would have sensibly the same value; it would have been for the other two about 57″, and 1′4″. The changes in the signs were found in the three wires, the first between 3 and 4 millemetres in height, the second between 13 and 14 millimetres. The negatively magnetized needles were affected a little less on the thickest wire. On being acted upon singly, the two latter needles gave, with an equal discharge, changes in the signs of the magnetism four times. The very fine wire only exhibited them twice, one at $4^{mil.}$, the other at about $11^{mil.}$ in height.

The equality of the action which took place at all points of a similar wire conductor, still subsists then almost wholly throughout the extent of a circle composed of several wires of different diameter, at least at some distance from the points of junction. The form of the periods is the same on all, saving the slight displacements in the position of the changes of the sign, and the same as on a wire equal in length to the sum of all the lengths, and of a diameter that is intermediate between all the diameters. If one of the wires be very fine, whatever place it occupies in relation to the others, it communicates to them nearly its own properties, whilst its manner of acting only undergoes feeble modifications. Circumstances transpire with regard to this wire, as if it was lengthened a very little, but with regard to the others as if their extent was much augmented. The invariable conductors of a battery, then, ought to be sufficiently large to permit of their influence being neglected.

It is not necessary to understand what has just been said of the equality of action observed in a complete circuit, relating to a system of wires of very different diameters, comparable to the distances where the changes of sign take place; neither to a case where *during* the discharge one of the wires changes its state and becomes entirely volatilized.

With a view of examining whether the force of the magnetism of a discharge would be modified by a resistance which, in the middle of each wire would tend to hinder the conducting wire from dilating itself, or is opposed to small transverse movements, I filled a crystal tube, whose exterior diameter was nearly four millimetres, and the interior diameter four-tenths of a millimetre, with mercury. The ends were closed by two brass wires, fixed by a little wax, and in contact with the mercury. With regard to the pressure exercised during the discharge, the effect has been to separate the tube into two, precisely in the middle of its length, to break it very near the extremities, and to furrow it symmetrically from that part of the middle to the quarters and a little beyond, by oblique fissures, such as are presented by the fillets of two helices turning in contrary directions. The other two quarters of the tube up to the extremities were whole. The pressure had even been a little stronger at the quarters, for at these two points, the mercury had escaped in very fine drops, showing that it had been driven out more abundantly at the two ends and in the middle. It is necessary to avoid the small bubbles of air or water interrupting the metallic fillet: this will suffice to determine the rupture at the two points of separation.

Under the relation of magnetism, the series presents a notable irregularity. Opposite the point of rupture at the middle of the tube a needle was found very feebly magnetized between two needles that were strongly magnetized. This experiment was not repeated.

When a very fine brass or platinum wire, of such a length that the magnetism produced by a strong discharge will present changes in the sign, and the wire is broken by the current, the magnetic effects, except perhaps at a very small distance from the point of rupture, remain the same as if the wire still remained extended.

After having passed a very strong discharge through a wire conductor, we no longer obtained on this wire precisely the same series of magnetic intensities for quantities of electricity that are sensibly equal. This alteration, which is very feeble, depends perhaps sometimes on a superficial oxidation, and ought to agree in general with the degree of annealing which the metal undergoes during a discharge at a high temperature.

Hitherto I have supposed all the needles of the same diameter, $\frac{1}{4}$ of a millimetre, and of the same degree of temper, the rolled temper. These two circumstances, temper and the diameter, have the greatest influence on the direction and the intensity of the magnetism which a discharge may produce and a conductor give, whilst as we have seen, the length of the needles has scarcely any influence.

As an example of the manner in which these *maxima* and the changes of sign in the magnetism are displaced by the degree of temper, I transcribe the following series, obtained on on a brass wire of $\frac{1}{4}$ of a millimetre in diameter, and by a single discharge, the

needles were all 15 millimetres long and 0 $^{mil.}$ 30 in diameter. They had all been cut from the same piece of steel.*

First Series. Needles tempered hard.

	Distance from the wire during the discharge. Mill.		Duration of 60 oscillations. m. s.		Direction of the magnetism.
1	0 6	....	1 22 8	....	Positive.
2	1 6	....	1 13 4	....	do.
3	2 6	....	1 20 0	....	do.
4	3 9	....	2 14 5	....	do.
5	4 9	....	1 42 0	....	Negative.
6	6 3	....	1 13 6	....	do.
7	7 6	....	1 5 8	....	do.
8	9 8	....	1 5 6	....	do.
9	10 8	....	1 0 8	....	do.
10	12 4	....	1 35 4	....	do.
11	14 8	....	2 14 4	....	Positive.
12	45 0	....	0 42 2	....	do.

Second Series. Needles untempered: flexible.

1	0$^{m.}$ 0	....	1′ 58″ 5	....	Positive.
2	1 2	....	3 50 0	....	do.
3	2 4	....	1 49 5	....	do.
4	3 8	....	1 18 0	....	do.
5	5 3	....	1 1 2	....	do.
6	6 3	....	0 56 4	....	do.
7	7 3	....	0 51 6	....	do.
8	9 4	....	0 50 2	....	do.
9	10 3	....	0 48 4	....	do.
10	12 8	....	0 46 8	....	do.
11	15 4	....	0 47 2	....	do.
12	42 0	....	1 10 6	....	do.

The needles tempered hard presented two changes in the sign; the untempered needles only presented at some distance from the wire a *maximum* of intensity. The *maximum* of the hard tempered needles is very much further from the wire, and at a higher value than that of the flexible needles.

In general the magnetism in the hard tempered state is a state of equilibrium between the greatest forces and resistances: a feeble exterior cause alters it with greater difficulty. In this state the needles acquire a more elevated *maximum* either in one direction or the contrary one, attaining it by a less discharge, and only commence

* I have sought to find whether, as M. De la Rive and M. Marianini have remarked on wires which have served to establish the communication between the poles of a pile, a platinum wire which has transmitted a strong discharge would give, when its extremities are plunged into a liquid conductor, any traces of an electric current. I could not observe any: but the apparatus I made use of was not sufficiently sensible, and this experiment merits a repetition.

losing it in a sensible manner with stronger discharges, and suddenly changing the sign. The mode of magnetizing due to M. Arago, thus offers the means of comparing, with exactness, the different degrees of the coercive force, which augments nearly in proportion as the *maximum* of magnetic intensity, the state of saturation, is itself increased, but which may still vary considerably when this *maximum* is very little changed.

The influence of the diameter of the needles on the magnetism which they receive cannot be disengaged from other causes, which, such as the degree of temper, make the results vary; for the interior particles of a thick needle cannot have taken in the sudden cooling the same disposition as the exterior ones. Be it as it may, I extract from a series made with needles of three different thicknesses, and of 15 mill. in length, on a brass wire of 0 mill. 125 ($\frac{1}{8}$ mill.), the following values of magnetic intensities at equal heights:—

Distance of the axis of the needles from the wire during the discharge.	Duration of 60 oscillations.		
	Needle of 0m. 30 in diam.	Needle of 0m. 80 in diam.	Needle of 1m. 75 in diam.
Mill.	m. s.	m. s.	m. s.
1 8	1 10 pos.	2 0 pos.	1 39 pos.
17 0	58 neg.	59 id.	1 46 id.
27 0	58 pos.	1 5 id.	57 id.
Maximum of intensity of needles magnetised by magnets	25	31	44

In place of the changes of sign presented by the thin needles, the mean needles only presented a *minimum* in contact with the wire even, and the largest needles a continued decrease of intensity in proportion as they were further removed from this wire.

The series presented by the mean needles and the thickest ones, are those which were obtained with the small needles with discharges more and more weak; with discharges more and more strong the thick needles themselves exhibited changes in the sign.

The phenomenon of the reversement of the poles produced in the needles of the compass by thunder may already be explained by known facts; since the fluid, according as it passed on one side or the other of these needles, all things beside being equal, ought to magnetise them in opposite directions. But this phenomenon still admits another explanation, founded on the preceding facts; for the fluid always passing on the same side of the needle, the direction of the magnetism still depends on the distance and the intensity of the discharge. We may even remark that compass needles are found in circumstances which facilitate the production of alternations of the contrary magnetisms. They are of very small substance, which places them almost in the state of isolated wires of a small diameter, and receiving the hard temper.

On the Action of Discharges Transmitted by Wire Conductors Wound as Helices.

The needles I have employed in the following experiments were fifteen millimetres long and quarter of a millimetre in diameter: they were tempered hard.

I rolled into a helix, on a hollow cylinder of dry wood of nine cent. long, and of about 0^{m} , 5 in diameter, a brass wire of $0^{mil.}$, 180 in thickness. The step or pitch of the helix was about 3 mil. in height. With a length of wire of $0^{met.}$, 80, the same needles, placed successively in the middle of the cylinder, in the direction of its axis, were magnetized by discharges, gradually increased. I here give for each needle after being magnetized, and in the order of the increasing intensity of the discharges, the duration of sixty oscillations: + 25″,6; + 56″,8; — 38″,2; — 25″,5; + 28″,9; + 27″,1; — 42″,0; — 33″,1; — 57″,6; + 27″,8; + 23″,0; + 34″,6; — 1″,15; + 31″,3. The sign + indicates that the needle was magnetized in the direction in which the voltaic current was transmitted, and which I have hitherto called *positively magnetized*; the sign — indicates a contrary direction of magnetism.

This series presents to us six changes in the sign. The second discharge, which corresponds to the number + 56″,8, was equivalent for the quantity of electric fluid to that of an ordinary Leyden jar. I could with difficulty perceive the luminous point drawn from the battery.

We should perhaps obtain with a less length of wire a greater number of changes in the direction in the magnetism. When on the contrary the wire is lengthened, the helically wound part remaining the same, not only does it require a greater force to obtain the first reversing of the poles, but in place of the changes of sign following, we only find some variations of magnetic intensity: thus for discharges equal to those of the preceding series, and a similar wire, but of double the length ($1^{m.}$, 6), the corresponding numbers to the 3rd, 5th, 7th, 9th, 11th, 12th, and 13th observations of that series are +25″,0; —31″,9; +31″,0; +25″,6; +51″,6; +54″,6; + 46″,0. Beyond that the magnetism continues to augment in the same direction.

The period which is formed by the four latter values is that which becomes by the effect produced by lengthening the wire, the period comprised between the sixth and tenth observations in the first series. We see how it differs from it. However, these two periods would become identical, if the effects due to the lengthening of the wire could be compensated for by an increase in the intensity of the discharges. This compensation, therefore, cannot take place. We must not forget that in the two cases that part of the wire which acts immediately on the needles is exactly the same. A similar conclusion has already been presented in the examination of the action of rectilinear conductors.

I shall not here report the different series of magnetic intensities obtained by employing lengths of the same wire, gradually increased,

whilst with the wire $0^{met.}$, 80 long, the first reversing of the poles takes place with a quantity of fluid which does not much surpass the charge of a simple Leyden jar. It is only with a very strong discharge of a battery of twenty-two feet surface that we obtain the same effect with a wire of equal diameter, and of eleven metres in length, the helically wound part being the same in both cases. But this wire has given needles magnetized to saturation, making sixty oscillations in a little less than 23″, as well in one direction as the other.

The pitch of a helix always remaining the same, a brass wire of $0^{mil.}$, 09 in diameter, and $6^{mil.}$, 5 long, no longer gave a reversion of the poles. Nor could I obtain it with a silver wire of $\frac{1}{40}$ of a millimetre in diameter, and 0, 25 millimetres long, from the most feeble discharges up to those which reduced it completely into smoke. The *maximum* of magnetic intensity was with wires in a state of saturation.

I brought into communication by one of their extremities the helix of this same silver wire and a similar helix formed of a copper wire considerably larger. A discharge passing from one helix to the other produced changes in the direction of the magnetism in the second, when even the very fine wire was completely vapourized, and had so much power that it constantly magnetized in the same direction the wires submitted to its direct action. Also in the case where the diameters of the wires composing a circuit are very different, where at least one of these diameters is of extreme tenuity and rather fine, when during the discharge one of these wires changes its state the action of different parts of the circuit cannot be equal.

In general, for the same wire on a similar helix, the first *maximum* augments in value when the wire is made longer, and diminishes when its diameter is increased. In the first case, it requires a greater force to obtain the first reversion of the poles or the period of variations of intensity which replace it; in the second it requires a less force. Any *maximum* whatever is as much more elevated as the limits of the period to which it belongs are more scattered.

I pass, however, now to the case where leaving the diameter and the length of the wire invariable, we successively change the length of the diameter and the pitch of the helix.

The length of the helices, when it is equal to seven or eight times their diameter, and two or three times greater than the length of the needles, has scarcely any appreciable influence on the length, or the intensity of the magnetism.

The experiments of M. Arago have shown, as I have already said at the commencement of this memoir, that similar needles disposed in any manner whatever in the interior of a large helix, at least at some distance from its extremities, all receive parallel to its axis the same degree of magnetization;* and likewise

* This is also true, is whatever way the needles are magnetized.

this degree of magnetization is sensibly the same in two helices of different diametres, provided that they be sufficiently long and their pitch be equal and sufficiently short. When the spirals are a little removed from each other, to 3 millimetres for example, the helices have much greater action than when their diameter is smaller, but this increase of energy is extremely feeble. It is almost insensible, and may be altogether neglected with a pitch a moiety less, 1·5 millimetres. The most exact means of proving these small differences is by placing one in the other, two helices which turn in the same direction, as was done by M. Arago, and to bring into communication by their extremities the wires which they cover. The current then traverses them in contrary directions; they act in contrary directions on the needles placed in the interior helix, and these needles, either remaining unmagnetized or only acquiring, with very strong discharges, a feeble magnetism in the direction which the helix of the smallest diameter gives them. However, this magnetism increases little by little with the intensity of the discharge.

If the two helices, placed one in the other, and always of the same pitch, in place of turning in the same direction, turned in contrary directions, the one *dextrosum*, the other *sinistrosum*, the current traverses them longitudinally in oppositedirections and transversely; their actions, always pretty nearly equal, are added together, instead of being destroyed as in the preceding case. We may thus measure double, triple, or quadruple forces. It is sufficient if we make the discharge arrive at first at a simple helix, from that to a system of two, three, or four helices of the same pitch, one enclosed in the other, and alternately turning in contrary directions. The mutual action of helices which envelope each other is here, however, as we shall hereafter see, a cause of error, but this error is very small, if the wire of which they are formed is sufficiently fine.

Here I give, for electric charges of which the intensity may be represented in the gross by the numbers 2, 3, 4, 4½, and 5, the durations of oscillations measured by the magnetism of magnetized needles; the one in the simple helix, of which I before made use of, the others in the system of two helices of the same pitch, which I shall call, for the sake of brevity, the *double helix*.

Intensities of electrical charges.	During 60 oscillations.						
	Simple helix.			Double helix.			
		s.			m.	s.	
2	+	23	5	+	0	22	9
3	+	22	9	+	0	30	4
4	+	27	2	+	1	2	0
4½	+	31	8	—	1	17	0
5	—	32	8	—	0	23	5

We see already by these estimates, inasmuch as they are still very large, not only that the action of a double discharge is very different to double the action of a simple discharge; but further, that the

relation of these two forces is variable, and depends on their absolute intensity.

A double helix acts sensibly as a simple helix of a pitch a moiety shorter, the length of wire being the same for them both.

We may be assured that if a portion of a discharge is passed from one spiral to another, without following the contrary, this portion may be altogether neglected, at least for the heights of pitch that I have employed. It will suffice if we isolate with gum lac the spirals of an helix for a short extent, in any part of its length, we obtain on passing a discharge through this helix, exactly equal magnetic intensities in both of the moieties, whatever be the direction of the magnetism; whether it attains its *maximum*, or whether it be almost null.

Since in the whole interior of a helix similar needles are equally magnetized, every point of these needles are submitted to equal actions. The differences of magnetization which the different parts of the same needle present must only be attributed to the reaction of these parts upon each other. In order to appreciate the influence of these reactions, I took needles of the same temper and of the same diameter, but of three lengths: $15^{mill.}$, $10^{mill.}$, and $5^{mill.}$. I place in one and the same helix three new needles, one of each length. After having magnetized them they were made to oscillate. The mean needles were afterwards broken in two; those of fifteen millimetres into three equal parts, and these fragments, equal to small needles, were made to oscillate. These are the effects then remarked: the fragments of the same needle, equal to each other, are always magnetized in the same direction, that of the entire needle, and are equally magnetized; at least the differences, which are in general very small, always preserve the same sign from one extremity to the other, which solely announces a gradual inequality in the temper of the different points of the needle; an inequality almost inevitable, since in general these points are only successively plunged into the cold liquid.

The numbers which in the following table express the durations of the oscillations of the fragments of the same needle, are the means of durations almost equal, obtained by making them oscillate successively. The signs always indicate the direction of the magnetism.

	Duration of 60 oscillations.				
Electric charges.	Needles of 15 mill.	Needles of 10 mill.	Needles of 5 mill.	Fragments of 5 mill. of mean needles	Fragments of 5 mill. of large needles.
1........	+0′ 29″ 4	+0′ 22″ 4	+16″ 7	+14″ 0	+11″ 5
2........	—0 40 0	—0 30 6	—22 3	—16 6	—16 0
4........	—1 25 4	—1 6 8	null	—28 2	—24 5
6........	+0 30 3	+0 21 6	+14″ 2	+12 5	+11 7
....	+0 28 5	+0 20 0	+14 1	+12 5	+11 4
....	—0 28 7	—0 21 1	—14 7		
....	—0 26 9	—0 19 8	—14 8		
....	+0 33 6	+0 24 0	+16 6		

We see that the needles of different lengths are always magnetized in the same direction. Perhaps the shorter needles change the sign with forces a little less. It will be remarked that these needles of five millimetres are less magnetized in one direction as well as in the other, than the equal fragments of needles of ten millimetres; and that the latter are themselves less magnetized than the fragments of longer needles.

There can be no doubt but that in breaking the needles we alter the magnetism of their parts, but in a quantity which is not very considerable. One of the needles of fifteen millimetres, that made sixty oscillations in 28″, 5, was placed in a small glass tube of the same length as itself, extremely light, into which it entered with friction. This caused it to make the same number of oscillations in 37″, 8. It was then withdrawn; it was broken in three pieces, of which each separately made sixty oscillations in 11″, 8. The three pieces having been replaced, one after the other, in the small tube of glass, it then took 41″, in place of 37″, 8, to oscillate the same number of times.

I will cite, while noticing these latter researches, an analogous experiment. I magnetized, it is now a long time ago, to saturation, with two strong magnets, an untempered steel needle of about one millimetre in diameter, and thirteen centimetres long, on which I had previously traced, at the end of each centimetre, with a fine line, annular marks of but slight depth, but sufficient to determine the rupture of the needle when a slight pressure was exercised on any part of it. I convinced myself that the entire needle had only one magnetic centre. I then broke it into thirteen equal parts. The extreme fragments were then smaller than the distance of the poles from the two ends of the needle. Their magnetic distribution changed in a manner almost instantaneously. For after some minutes, and excepting some very feeble and irregular differences of intensity, all the fragments possessed the same quantity of magnetism, and almost the *maximum* to which it was possible they could obtain. The entire needle made sixty oscillations in 3″, 16′, 3. The fragments of that part from the north pole to the centre, made the same number of oscillations in 47″, 2; 49″, 2; 48″, 8; 47″, 2; 47″, 3; 50″, 2; 48″, 0, fragment from the middle; the following fragment 47″, 0. In a state of saturation, these fragments made sixty oscillations in 44″, and 46″.

I now return to the phenomena of magnetism produced by electricity. In what has been already said, I have supposed that all discharges, the feeblest as well as the strongest, being drawn from the same battery, that equal charges corresponded to the same tensions: In giving to electrized surfaces of very different extents, and gradually decreasing in proportion, that with equal charges the tension is more elevated. I found, by some trials, that the *maximum* of magnetic intensity, the helix and wire being the same, have a less value. Now has decrease any limit? How does the influence of tension combine with the influence of the length of the wires: with

their diameter, and the distance between the spirals? These elements of the question I have yet to study. In fact I have only spoken of the effects produced by an unique discharge. I ought as the basis of a possible explication, point out the effects obtained in helices by means of successive discharges. If these discharges are very feeble, such as simple sparks drawn from a machine, and very near, their action is added to each other, with certain limits of magnetization, which depends on the nature of the needles. Any number whatever of the same sparks will not raise it beyond the limit of magnetism already produced. The resistance which is opposed to its development increases then with the magnetism already developed. This resistance which is, perhaps, great enough in needles still unmagnetized to prevent a continuous series of sparks sufficiently feeble from ever exerting any sensible influence.

If instead of increasing, the sparks traversing the wire in a contrary direction tend to produce opposite effects, the resistance, in proportion as we approach the first *maximum* of magnetization, becomes more and more unequal in the two directions, and the force necessary to destroy the magnetism produced is only a very feeble portion of the force lost when we seek to augment it.

And again: as soon as the discharges pass beyond the degree of intensity capable of giving the needles this first *maximum* of magnetism, which may be very far from the state of saturation, the direction and the degree of magnetism which they give them are very near, and more and more independent of the magnetic state in which those needles were found before the experiment.

On the action of other bodies, as Steel and Iron, submitted to the action of electric discharges, but isolated from the conductor which they traverse.

One of the first observations of M. Arago, at the time of his researches on magnetism, was, that the influence which magnetises iron and steel, very different from that of other electric actions, is transmitted through wood, glass, and other insulating substances, without undergoing any appreciable change. Two needles (*Ann. de Chem. et de Phys.*, 1820), placed in the same helix, the one enclosed in a crystal tube, the other out of this tube, received exactly the same quantity of magnetism. These trials, which M. Arago had intended to have varied and extended, were pursued no farther until the time when he discovered the very remarkable action that all the substances, and especially the metals in a state of movement, exercised on the needle. I expressed to him, a short time after, the desire I had to know what might be the influenc eof this latter class of bodies on the magnetism developed by the electric currents, and he was very desirous to engage me to follow out a class o fresearches which he himself had intended to repeat.

I at first placed two needles in a helix, the one without envelope, the other enveloped in a thick cylinder of red copper, isolated from the conductor. The effect of a discharge which strongly magnetized

the first was insensible on the second. There was nothing transmitted through the copper. I substituted a needle already magnetized for the needle without magnetism enclosed in the metallic tube. I then placed it in such a manner that a new discharge ought to change its poles, or at least to enfeeble its magnetism, if the envelope did not destroy all its effects. The duration of the oscillations, measured before and after this trial, were found to be exactly the same. It is useless to say, that in order to eliminate the influence due to terrestial magnetism, the needles were always placed, during the discharge, in a direction perpendicular to the magnetic meridian.

I gradually diminished the thickness of this metallic envelope; the intensity of the discharges always remaining the same, the envelope needles commenced exhibiting an action more and more sensible. For a certain thickness the enclosed needle and that entirely naked, if I may be so allowed to express myself, were equally magnetized. For thickness gradually decreasing, the enveloped needle became the most strongly magnetized of the two, attaining a *maximum* of intensity, and then it afterwards approached anew, by successive diminutions, to the degree of magnetization of the other needle.

In proportion as the intensity of the discharges augment, the thickness of the metallic envelope with which the needle was covered, and the one that was not, received the same degree of magnetism, and may be gradually increased. At the same time the increase of magnetism due to a diminution of the thickness of the envelopes is more and more considerable. With very feeble discharges the thickness which has no action is very small.

These experiments were made with plates of pewter, rolled round the needles. This disposition permitted the thickness of the envelope to be gradually diminished. I assured myself besides, that a cylinder of cast pewter and a cylinder formed of leaves of rolled pewter, as nearly equal in volume and in weight as the greater density of the plate pewter would permit, exercised sensibly the same action.

The eighth part of leaf of beaten silver, weighing $0^{g}, 005$, was rolled round a needle of two centimetres in length, and five times heavier. This very thin envelope, when submitted to a discharge from a Leyden jar, raised by one-third the degree of magnetism which the same discharge communicated to a similar needle by its direct action.

When in a given helix, the first *maximum* attained by the needles without envelope is very far from the state of saturation; with discharges increasing in strength, and which magnetize them in decreasing ratios, the maximum of similar needles in a convenient envelope continues considerably to raise itself beyond the *maximum* of the first ones. It will even become raised when these are magnetized in a contrary direction. This is a new proof that even at high tensions the electric fluid does not pass, at least in appreciable quantity, from one spiral to another of the helices, without following the turnings therein.

Thus, two needles of two centimetres in length, magnetized, the one without envelope, the other in a small cylinder of pewter of two millimetres radius, by a single discharge, employed for the first 8′, 30″, the second 43″ only made sixty oscillations.

Three needles, of fifteen millimetres in length, and 0^{mil}, 4 in diameter, the one in a cylinder of copper of five millimetres radius, the second in a similar cylinder of brass, the third without envelope, received in the same helix, quantities of magnetism having for their measure the numbers + 2′, 35″; + 45″; — 1′ 52″, durations of sixty oscillation for each needle. The sign —, which precedes the latter value, indicates that the needle without envelope was magnetized in a contrary direction to the others. The discharge was very strong, and the cylinder of copper had almost destroyed the action which the pewter on the contrary had considerably raised.

By comparing two metallic tubes of equal thickness and length, but of different radius, and consequently of different masses, we find that the largest, that whose mass is the most considerable, exercises the greatest action. If two tubes have the same diameter, the same thicknesses, and unequal lengths, the shortest is that whose action has the most influence. Of two helices, on the contrary, the longer is the most powerful. I suppose the length of the tubes sufficiently large in relation to that of the needles.

Here is an example. I compound two cylinders of pewter of three millimetres in thickness, one of sixty-five millimetres in length, the other 100 millimetres. Their action, with a rather weak discharge, were in the proportion of about three to one.

There must exist then between the thickness and the length of a metallic cylinder a certain relation, with which the influence of this cylinder is the greatest possible, under the action of a given discharge.

If a metallic tube is at the same time very long, and of an interior sufficiently great, the needles parallel at its axis receive quantities of magnetism sensibly equal in all the space enveloped by it, at least a short distance from its extremities.

We may change the nature of the isolating substance which separates the helix from the metallic envelope, and give to this envelope in the interior of the helix any position whatever, provided that the axis of their figure is always parallel, without changing the action of the metal on the needle which it encloses. On comparing these two experiments with experiments quite similar to those of M. Arago, on the direct magnetization of needles without envelopes we see that the electric movement acts in the same manner on any metal whatever, and on steel, which alone preserves its magnetism.

When we multiply concentrically around the needle alternating layers of conducting and non-conducting materials, the first actions do not appear sensibly modified by the fact of their insulation. There is no doubt, on the contrary, that their action is not very much enfeebled by the sections perpendicular to the axis of the figure, or in which these planes pass by this axis. In fact, envelopes of great

thickness, of fine copper filings, or even of iron, equal in weight to the cylinders of those metals which completely destroy the influence of a given discharge, scarcely at all modify this influence. We see then the analogy which exists between these results with the beautiful experiments of M. Arago, on the rotation of plates.

If in a metallic cylinder, for example, in a tube of glass, filled with mercury, needles parallel to its axis are placed at different distances from the surface, from the circumference to the centre, and we then compare them to a needle submitted without envelope in the same helix to the same discharge, we at first remark a gradual increase of intensity, a *maximum*, and then a diminution, which extends to the centre. If the discharge is weak enough, or the envelope sufficiently thick, there is a certain radius on which the sum of the actions of the metal is null. It is even very probable that, with electric discharges much stronger and metallic thicknesses proportionally greater, we should find several concentric surfaces where the action is null.

I here give some measures of magnetic intensities, obtained at different distances from the metallic surface, in a tube of glass of 10 $^{mill.}$ interior radius, filled with mercury :—

Distance of the exterior surface of the liquid... $1^{m.}$ 5; $2^{m.}$ 0; $2^{m.}$ 8; $4^{m.}$ 0; $6^{m.}$ 0; $12^{m.}$ 0.

Duration of 40 oscillations.................. 28″ 9; 28″ 8; 29″ 1; 30″ 8; 49″ 2; 1′ 22″ 7.

A needle magnetized with the same discharge, out of the influence of the mercury, made the same number of oscillations in 1′ 49″ 3: the needles were $2^{cent.}$ long, $0^{m.}$ 5 diameter.

The only metals that I have tried in very thick cylinders, iron, copper, pewter, and mercury, act with a decreasing energy. As to non-metallic conductors, such as nitric acid, sulphuric acid, and water, if their energy is not null, the experiments I have attempted were not sufficiently delicate to enable me to recognize it in a certain manner.

We have just seen how the metals, submitted to the influence of discharges in the interior of helices, modify the magnetism. It will be easy to conclude from that the manner in which these metallic plates, under the influence of discharges transmitted by a rectilinear wire conductor,* act on the steel needles disposed transversely to this wire; however, as this action presents two distinct cases, I now proceed to expose them separately. I suppose the needles to be in contact with the metallic surfaces, and the discharges inferior to those which, by their direct action, produce in steel, following the distance of the wire, opposite magnetic states.

1st. A large plate interposed between the conductor and the needles for very feeble discharges, considerably enfeebles the

* It is almost indifferent whether the needle be or be not isolated from the conductor; that is to say, if it is sufficiently isolated by the defect of absolute contact, which can only be established by means of a strong pressure.

magnetism, and augments it with stronger discharges. Thus, for a similar discharge, a thin plate and a thick one may produce contrary effects, and there is a certain thickness where the effect is null.

2nd. The needles put on the plate, between this place the wire; with very feeble discharges it augments their magnetism, and more so as its thickness is increased. There is a certain discharge with which a thick plate augments it and a thin plate diminishes it. With stronger discharges they are both enfeebled, the latter especially, and it finishes by giving to the needles a contrary magnetism to that which the current itself developes.

In general the two faces of the same plate exercise contrary actions.

When the discharge is strong enough to magnetize the needles by its direct action, in contrary directions following their distance from the wire, the magnetism, under the influence of the metals, results from several causes, each of which is submitted to these laws periodically different. The different parts of the metallic plates also pass, as well as the needles, through a series of opposite states, and in each of these states they act as a magnet possessed of an analogous polarity.

In searching to compare the different plates of different metals, of similar form and equal thickness, it was remarked that soon not only that the relations of their actions varied, but that the order of the series that was wished to be formed by it was found to be reversed. The red copper in thin plates acted less than the brass; in still thinner plates it finished by acting more. I here give some numerical values. The direction of the magnetism is always designated by the signs + and —. Three needles at two millimetres beneath the conductor, first, magnetised on a plate of glass, the duration of sixty oscillations: + 1′, 25″; second, on a plate of pewter, —1′, 28″; third, on a similar plate of copper, — 1′ 56″. With a stronger discharge and two thinner plates of copper —1′, 4″; pewter —1′, 34″. The difference between the action of tin and that of pewter is not greater than the difference between the actions of pewter and copper. I find the three following values with three similar needles; on the glass + 1′, 34″; on the tin, — 1′ 6″; on the pewter, 1′, 36″.

Silver acts almost like copper, gold has a much greater action.

The action of metallic envelopes is a means of studying what takes place in the different parts of the steel needles themselves during magnetization. It is, in fact, in the same manner as other metallic envelopes, that the exterior laminæ of the needles act in the interior parts, and this influence may differ materially from the magnetic action which they exercise at a later period, as particles magnetised in a permanent manner. The experiment I have reported on magnetism, produced at different distances from the surface, in a tube of glass filled with mercury, is an example of this class of researches. It would be necessary, to render it complete, to distribute the needles

throughout the whole mass and up to its extremities. The reaction of these needles on the metal which envelopes them, at least in a first approximation, may be neglected.

In proportion as with equal charges the tension diminishes, as is the case when we augment the extent of the electrized surfaces, the influence of metals during the discharge becomes more feeble. This influence will doubtless be sufficiently small under the action of a current of electricity, circulating without interruption and without sparks from the conductor to the cushions, in the frictional machines powerful enough to produce them, as M. Rodolfi has announced permanent magnetism in steel needles. This same current however may offer, by its action on the metals, characters which distinguish it from voltaic currents, as it is already distinguished by the greater extent of conductors of a very small diameter which it may traverse. I have not yet, for want of a convenient apparatus, verified these conjectures.

On Magnetism produced by Voltaic Currents.

The phenomena I have just exposed are reproduced and measured with facility. This is not the case with those I am about to describe. A great number of causes make them vary every moment. And again I have only been able to draw from researches still very incomplete, a small number of general results.

All the points of a conducting wire, equal and homogenous, traversed by a voltaic current, exercise equal actions.* If it is rectilinear, it magnetizes equally in all its lengths, at least for a length which is not very great and at some distance from its extremities, but it magnetizes very little if the pile is not very strong. I prefer rolling it from space to space in such a manner as to form several small helices of some turns each of it, similar and separated by any portions of figure and extent whatever. Similar needles are equally magnetized in each of these helices.

The magnetization by voltaic currents, is entirely developed in a very short time; it is sensible instantaneously, at least with very small needles. A feeble pile may act during a long time, if its intensity is not augmented, even on a needle not tempered, without changing in an appreciable manner the degree of magnetism which it gives to the needle at the moment of establishing the communication. This is true, even when the degree is very far from a state of saturation.

In general, in whatever manner the pile is varied, if the current does not change its direction during the time a needle is submitted to its action, the needle will be magnetized as if the pile had always had the strongest of these different intensities. The magnetism produced by a pile, then, may furnish very different indications from those which we draw from the finished deviations of an already magnetized needle.

* M. Becquerel has already proved this fact from another class of observations.

Independently of the slow variations of the continuous current which traverses a pile, the tension at the instant the communication is established, as at the moment at which it is broken, produces a visible spark, or at least a transmission of electricity analogous to the discharges procured from the frictional machines. The same effect may be repeated, at intervals more or less apart, every time that the communications are only imperfectly established, as by the contact of wires of copper and mercury, especially if the surfaces are moistened with a saline mixture.

We ought, then, to observe again, in the magnetization by the pile, the phenomena observed in the passage of an electric discharge, modified by the action of the continuous current and the smallness of the tension.

In proportion as the tension is augmented, and the conductibility diminished, that part of the effect of discharge is greater, the action of the current continues with less influence. Thus, with an apparatus of 20 pairs,* and a very bad conducting liquid, I have obtained in a most certain manner, since the needles several times presented the same results, a direction of magnetism contrary to that which gave the same pile the most lively excitement. Some needles, which passed a little beyond the extremity of the small helices were then magnetized in the same direction, and more strongly than the needles placed at the centre of the same helices. The contrary took place when the pile was more energetic. I have not however sufficiently observed this phenomenon to point out the cause of it, if this is not the possible cause. The dry piles ought to produce it easily.

It is especially in the action of other metals besides steel and iron, that the influence of small tensions is sensible. If two needles are placed in the same helix, the one without any envelope, the other encircled by a thick cylinder of copper of five millimetres radius, for example, an energetic current magnetizes them almost equally, and more so as the conductibility is greater. A feeble current gives them degrees of magnetism still more different; the communication of which we may renew and interrupt successively a great number of times, as the conductibility is more imperfect, and the tension stronger.

Thus with an apparatus of ten pairs, feebly excited, I find that by multiplying the immersions of wire in mercury, a very small needle may be magnetized in such a manner as to make sixty oscillations in 36″; a similar needle placed in a cylinder of copper, the same number of oscillations in 1′, 2″. The plates having been lowered into the liquid, and afterwards withdrawn, without having touched either the communication between the poles established

* The conducting wire was soldered to the extreme plates, and the needles placed before the immersion of the plates were not removed until it was withdrawn from the liquid.

previously, or the needles placed equally in advance, these nearly received the same degree of magnetism. Still there is in this case a sudden change in the electric movement, at the instant the plates are withdrawn from the liquid.

If we leave the plates of an apparatus a long time immersed, the difference of the magnetism of the two needles, the one without metallic envelope, the other enveloped, becomes greater and greater in proportion as the action is less active; doubtless because the influence of the small spark, though it does become enfeebled, diminishes relatively less than that of the continuous current. Thus three pairs of needles, with and without metallic envelopes, having been magnetized, the one at the commencement of the immersion of the pile, the second 8′ after, the third at the end of 20′, the differences of the times employed by the two needles of the same pair to make sixty oscillations were 15″, 48″, and 2′; the time employed by the needle without envelope being 2′, 52″; 2′, 55″; and 3′, 37″. In the state of saturation the duration of sixty oscillations is 2′, 38″.

The redeeming influence of the metallic envelopes increases a little with their thickness. I ought to search out, if in the case where a thick envelope enfeebles the magnetism, a thin envelope would augment it. The envelopes tried were perhaps too thin, their influence being insensible. The small difference of action of the two envelopes of unequal thickness is without doubt, the part of the effect due to the continuous current.

I am far from well recognizing the circumstances in which are produced, under the influence of voltaic currents, the actions I have just been speaking of. I have sometimes found that the thick metallic envelopes augmented the magnetism in a superior quantity to a quarter of these experiments. When that happened the needles were withdrawn from the helices, without at all changing the immersions of the plates or the communications. The conducting wire was soldered to the extreme elements, and each plate of zinc soldered to the plate of copper following. In truth there is here a circumstance which the discoveries M. Arago will not permit me to neglect: the displacement of the needles in relation to the wire conductor and to the copper cylinder.

Two needles enclosed, the one in a copper tube, the other in one of wood, and similarly placed between two magnets sufficiently feeble only to give a degree of magnetism far removed from the state of saturation, have always received degrees of magnetism sensibly equal. We ought in all cases to take care to provide against the effect due to the inclination of their magnetic axis.

I have been obliged to expose new facts independently of any explanation; I may now, however, be permitted to point out rapidly the consequences.

An electric discharge is a phenomenon of movement. This movement, is it a continuous transport of matter in a determined direction?

Then the alternative opposed magnetisms which we observe at different distances from a rectilinear conductor, or in a helix with gradually increasing discharges, would be due uniquely to mutual reactions of magnetic particles in the steel needles. The manner in which the action of a wire changes with its length appears to me to exclude this supposition.

The electric movement during the discharge, is it composed, on the contrary, of a series of oscillations transmitted from the wire* to the surrounding media, and soon recovered by the resistances which rapidly are borne away with the absolute quickness of the agitated particles?

All the phenomena conduce to this hypothesis, which makes to depend, not only the intensity but the direction of the magnetism, on laws following which the small movements were weakened in the wire, in the medium which surrounds it, and in the substance which receives the magnetism.

The oscillations in the wire will have quickness absolutely as much less as they spread themselves more and more rapidly as this wire is increased in length, thinner, and as the resistance proper to its nature becomes more considerable. We thus explain how it is that, with a rectilinear conductor and a given discharge, a length of wire which produces the strongest magnetism, if the length is less, the small movements diminish too slowly; if greater, their intensity is too much enfeebled.

Inasmuch as powerful metallic substances, as we have seen, sometimes increase and sometimes enfeeble the magnetism, it is sufficient that they weaken, in the two cases, the small movements propagated by the wire, and that their section be not simply proportional to the absolute quickness of these movements. It suffices then to admit, with these infinitely small displacements, what the discovery of M. Arago puts in evidence with oscillations of a limited amplitude.

Under the influence of the pile, the relative phenomena, whether of direct magnetism or of the action of metallic envelopes, are analogous to those presented by the discharges of ordinary electricity. When we destroy the communication whilst the needles are submitted to the action of a wire conductor, it is natural to think that the equilibrium is re-established in this wire by a series of small movements analogous to those excited by the discharge. But when the needles are taken away from the voltaic action, without there being a sudden interruption of the circuit, the influence which a metallic envelope several times exercised to augment the magnetism, seemed to indicate in the closed circuit the existence of two contrary currents, animated by very different speeds, or rather by small movements, the duration and quickness of which were in the two opposite direc-

* The wire, which may be entirely insulated from the ground and from the battery, receives and transmits the discharge by two sparks without the magnetic effects already described taking place.

tions. A pendulum oscillating in the medium, whose density decreases continually from one extremity to the other of the axis it travels over, will be an example of this kind of movement. Does not the contact of the two metals present such a medium? This hypothesis, which may give birth to some researches, proper either to confirm or destroy it, can only acquire additional weight by new facts.

In applying to the experiments contained in this memoir the considerations which I limit myself only to indicate here, I have not found anything for which they do not easily render a reason. But it would be too long, and perhaps misplaced, to enter on the subject in a first work in this theoretical discussion. New researches, which it has suggested to me, will, I hope, furnish me with an occasion of returning to it and the means of developing it.

On the Chemical Statics of Organized Beings. Extract from the concluding Lecture, in L'Ecole de Médicine in Paris. By M. Dumas.

Life, whose painful mysteries you are called upon to fathom, exhibits among its phenomena some which are manifestly connected with the forces that inanimate nature herself brings into action, others which emanate from a more elevated source, less within the reach of our boldest stretch of thought.

I. Plants, animals, man, contain matter. Whence comes it? What does it effect in their tissues and in the fluids which bathe them? What becomes of it when death breaks the bonds by which its different parts were so closely united?

These are the questions which we touched upon together, at first with hesitation, for the problem might be far above the powers of modern chemistry; we afterwards considered them with somewhat more confidence, as we felt from the silent and inward assent of our understandings that the path was sure, and that we could descry the goal gradually standing out, clear of all that obstructed our vision. If from these labours which you have witnessed, or, I should rather say, in which you have taken part; if from this scientific effort there have arisen some general views, some simple formulæ, it is my duty to become their historian; but allow me the pleasure of adding, that they belong to you, that they belong to our school, the intelligence of which has been exercised on this new ground. It is the ardour with which you have followed me in this career that has given me strength to pursue it; it is your interest which has sustained me; your curiosity which has awakened mine: your confidence which has made me see, and which proves to me at this moment that we are still in the path of truth.

These remarks will remind you of the wonder with which we found that, of the numerous elements of modern chemistry, organic

nature borrows but a very small number; that from these vegetable or animal matters, now multiplied to infinity, general physiology borrows not more than from ten or twelve species; and that all the phenomena of life so complicated in appearance, belong, essentially, to a general formulæ so simple, that, so to speak, in a few words the whole is stated, the whole summed up, the whole foreseen.

Have we not proved in fact, by a multitude of results, that animals constitute, in a chemical point of view, a real apparatus for combustion, by means of which, burnt carbon incessantly returns to the atmosphere under the form of carbonic acid; in which hydrogen burnt without ceasing, on its part continually engenders water; whence in fine, free azote is incessantly exhaled by respiration, and azote in the state of oxide of ammonium, by the urine?

Thus from the animal kingdom, considered collectively, constantly escape carbonic acid, water in the state of vapour, azote, and oxide of ammonium: simple substances, and few in number; the formation of which is strictly connected with the history of the air itself. Have we not, on the other hand, proved that plants, in their normal life, decompose carbonic acid for the purpose of fixing its carbon and disengaging its oxygen; that they decompose water to combine with its hydrogen, and to disengage, also, its oxygen; that in fine, they sometimes borrow azote directly from the air, and sometimes indirectly from the oxide of ammonium, or from nitric acid; thus working in every case, in a manner the inverse of that which is peculiar to animals? If the animal kingdom constitutes an immense apparatus for combustion, the vegetable kingdom, in its turn constitutes an immense apparatus for reduction, in which reduced carbonic acid yields its carbon, reduced water its hydrogen, and in which, also, reduced oxide of ammonium and nitric acid yield their ammonium, or their azote.

If animals, then, continually produce carbonic acid, water, azote, oxide of ammonium; plants incessantly consume oxide of ammonium, azote, water, carbonic acid. What the one class of beings gives to the air, the other takes back from it; so that to take these facts at the loftiest point of view of terrestrial physics, we must say that, as to their truly organic elements, plants and animals spring from air, are nothing but condensed air; and that in order to form a just and true idea of the constitution of the atmosphere at the epochs which preceeded the birth of the first organized beings on the surface of the globe, there must be placed to the account of the air, by calculation, that carbonic acid and azote whose elements have been appropriated by plants and animals. Thus plants and animals come from the air, and thus to it they return; they are real dependencies of the atmosphere.

Plants, then, incessantly take from the air what is given to it by animals; that is to say, carbon, hydrogen, and azote, or rather, carbonic acid, water, and ammonia.

It now remains to be stated, how in their turn, animals acquire those elements which they restore to the atmosphere; and we can-

not see without admiring the sublime simplicity of all these laws of nature, that animals always borrow these elements from plants themselves.

We have, indeed, ascertained, from the most satisfactory results, that animals do not create true organic matters, but that they destroy them; that plants on the contrary, habitually create these same matters, and that they destroy but few of them; and that in order to effectuate particular and determinate conditions.

Thus it is in the vegetable kingdom that the great laboratory of organic life resides; there it is that the vegetable and animal matters are formed, and they are there produced at the cost of the air.

From vegetables these matters pass ready-formed into the herbivorous animals, which destroy a portion of them, and accumulate the remainder in their tissues.

From herbiverous animals, they pass ready formed into the carniverous animals, who destroy or retain some of them, according to their wants.

Lastly, during the life of these animals, or after their death, these organic matters, as they are destroyed, return to the atmosphere whence they proceeded.

Thus closes this mysterious circle of organic life at the surface of the globe. The air contains, or engenders, oxidized products, as carbonic acid, water, nitric acid, oxide of ammonium. Plants, constituting true reducing apparatus, possess themselves of their radicals, carbon, hydrogen, azote, ammonium. With these radicals they form all the organic, or organizable matters, which they yield to animals. These, forming in their turn, true apparatus for combustion, re-produce carbonic acid, water, oxide of ammonium, and nitric acid, which return to the air, to produce anew, and through endless ages the same phenomena.

And if we add to this picture, already, from its simplicity and its grandeur so striking, the indisputable function of the solar light, which alone has the power of putting in motion this immense apparatus, this apparatus never yet imitated, constituted of the vegetable kingdom, and in which is accomplished the reduction of the oxidized products of air, we shall be struck with the import of these words of Lavoiser:

"Organization, sensation, spontaneous movement, life, exist only at the surface of the earth, and in places exposed to the light. It would seem that the fable of the torch of Prometheus was the expression of a philosophic truth which had not escaped the ancients. Without light, nature was without life, was dead and inanimate: by the gift of light, a beneficient God spread upon the surface of the earth organization, feeling, and thought."

These words are as true as they are beautiful. If feeling and thought, if the noblest faculties of the soul and of the intellect, have need for their manifestation of a material covering, to plants is assigned the framing of its web with the elements which they borrow

from the air, and under the influence of the light which the sun, its inexhaustible source, pours in unceasing floods upon the surface of the globe.

And as if in these great phenomena all must be connected with causes which appear the most distant from them, we must, moreover, remark how the oxide of ammonium, the nitric acid, from which plants borrow a part of their azote, are themselves almost always derived from the action of the great electric sparks which flash forth in stormy clouds, and which (furrowing the air through a vast extent) produce there the nitrate of ammonia which analysis detects in it.

Thus, from the craters of those volcanoes whose convulsions so often agitate the crust of the globe, continually escapes carbonic acid, the principal nutriment of plants; from the atmosphere flashing with lightnings, and from the midst of the tempest itself, there descends upon the earth the other and no less indispensable nutriment of plants, that whence they derive almost all their azote, the nitrate of ammonia contained in storm showers. Might not this be called, as it were, an idea of that chaos of which the Bible speaks, of those times of disorder and of tumult of the elements, which preceded the appearance of organised beings upon the earth?

But scarcely are the carbonic acid and the nitrate of ammonia produced, than a form more calm, although not of inferior energy, comes to put them in action; it is light. Through her influence, the carbonic acid yields its carbon, the water its hydrogen, and the nitrate of ammonia its azote. These elements unite, organized matters form, and the earth puts on its rich carpet of verdure.

It is, then, by continually absorbing a real force, the light and the heat emanating from the sun, that plants perform their functions, and that they produce this immense quantity of organised, or organic matter—pasture destined for the consumption of the animal kingdom. And if we add, that animals on their part produce heat and force in consuming what the vegetable kingdom* has produced, and has slowly accumulated, does it not seem that the ultimate end of all these phenomena, their most general formula, reveals itself to our sight?

The atmosphere appears to us as containing the primary substances of all organization, volcanoes, and storms; as the laboratories in which were first produced the carbonic acid and the nitrate of ammonia which life required for its manifestation, or its multiplication.

In aid of these comes light, and developes the vegetable kingdom, immense producer of organic matter; plants absorb the chemical force which they derive from the sun to decompose carbonic acid, water, and nitrate of ammonia; as if plants realised a reducing

* "*Le règne animal*," in the original; but this is obviously an error.

apparatus superior to all those with which we are acquainted, for none of these would decompose carbonic acid in the cold.

Next come animals, consumers of matter and producers of heat and force, true apparatus for combustion. It is in them undoubtedly that organized matter puts on its highest expression. But it is not without suffering from it that it becomes the instrument of sensation and of thought; under this influence organized matter undergoes combustion; and in reproducing the heat and the electricity which produce our strength, and which are the measure of its power, these organized, or organic matters become annihilated, in order to return to the atmosphere whence they came. Thus the atmosphere constitutes the mysterious link which binds the vegetable to the animal kingdom.

Vegetables, then, absorb heat and accumulate matter which they have the power to organize.

Animals, through whom this organized matter only passes, burn or consume it, in order to produce in its aid the heat and the different powers which their movement turns to account.

Suffer me, therefore, if, borrowing from modern science an image of sufficient magnitude to bear comparison with these great phenomena, we should liken the existing vegetation (truly a storehouse in which animal life is fed,) to that other storehouse of carbon constituted of the ancient deposits of pit-coal, and which, burnt by the genius of Papin and Watt, also produces carbonic acid, water, heat, motion: one might almost say, life and intelligence.

In our view, therefore, the vegetable kingdom will constitute an immense *depot* of combustible matter, destined to be consumed by the animal kingdom, and in which the latter finds the source of the heat of the locomotive powers of which it avails itself.

Thus we observe a common tie between the two kingdoms, the atmosphere; four elements in plants and in animals—carbon, hydrogen, azote, and oxygen; a very small number of forms under which vegetables accumulate them, and under which animals consume them; some very simple laws, which their connexion simplifies still more: such would be the picture of the most elevated state of organic chemistry which would result from our conferences of the present year.

You, like myself, have felt, that before separating we have need of collecting our thoughts, of fixing with precision all the facts, of bringing together and summing up the opinions which explain and develope these great principles; lastly, that it was useful, as regarded your future studies, to give you in writing, and in a clearer form, the expression of these views, which were partly brought into existence under the stimulus of your presence, and consequently reduced into form with the hesitation which so often accompany the first enunciation of our thoughts.

Since [the causes of] all the phenomena of life are exerted upon matters which have for their base carbon, hydrogen, azote,

oxygen; since these matters pass over from the animal kingdom to the vegetable kingdom, by intermediary forms, carbonic acid, water, and the oxide of ammonium; lastly, since air is the source whence the vegetable kingdom is fed, and the reservoir in which the animal kingdom is annihilated, we are led to take a rapid survey of these different bodies with a special view to general physiology.

Composition of Water.

Water is incessantly formed and decomposed in animals and plants; to appreciate what results from this, let us first see how it is composed. Some experiments founded on the direct combustion of hydrogen, and in which I have produced more than two pounds of artificial water: experiments which are in truth very difficult and very delicate, but in which any errors would be unimportant with regard to the circumstances which we are engaged upon: make it very probable that water is formed, in weight of 1 part hydrogen, and 8 parts oxygen, and that these whole and simple numbers express the true relation according to which these two elements combine to form water.

As substances always present themselves to the eyes of the chemist by molecules, as he always endeavours to connect in his thoughts, with the name of each substance the weight of the molecule, the simplicity of this relation is not unimportant.

In fact, each molecule of water being formed of one molecule of hydrogen, and one molecule of oxygen, we arrive at these simple numbers, which cannot be forgotten.

A molecule of hydrogen weighs 1; a molecule of oxygen weighs 8; and a molecule of water weighs 9.

Composition of Carbonic Acid.

Carbonic acid keeps incessantly forming in animals, and is continually undergoing decomposition in plants; its composition, therefore, deserves a special notice in its turn.

Now, carbonic acid, like water, is represented by the most simple numbers. Experiments founded on the direct combustion of the diamond, and on its conversion into carbonic acid, have proved to me that this acid is formed of the combination of 6 parts by weight of carbon, and 16 parts by weight of oxygen.

We are, therefore, led to represent carbonic acid as being formed of one molecule of carbon weighing 6, and two molecules of oxygen weighing 16, which constitute one molecule of carbonic acid weighing 22.

Composition of Ammonia.

Lastly, ammonia, in its turn, seems formed in whole numbers of 3 parts of hydrogen and 14 of azote, which may be represented by 3 molecules of hydrogen weighing 3, and by 1 molecule of azote weighing 14.

Thus, as if the better to show all her power, Nature operates in the business of organization only upon a very small number of elements, combined in the most simple proportions.

The atomic system of the physiologist revolves on these four numbers—1, 6, 7, 8. 1 is the molecule of hydrogen; 6 that of carbon, 7 or twice 7, *i. e.* 14, that of azote; 8 that of oxygen.

These numbers should always be associated with these names, because for the chemist there can exist no abstract hydrogen, nor carbon, nor azote, nor oxygen. They are beings in their reality which he has always in view; it is of their molecules that he always speaks; and to him the word hydrogen depicts a molecule which weighs 1; and the word carbon, a molecule which weighs 6; and the word oxygen, a molecule which weighs 8.

Composition of the Air.

Does atmospheric air, which performs so great a part in organic nature, also possess as simple a composition as water, carbonic acid, and ammonia? This is the question which M. Boussingault and I have recently been studying. Now we have found that, as the greater number of chemists have thought, and contrary to the opinion of Dr. Prout, to whom chemistry owes so many ingenious views, air is a mixture, a true mixture.

In weight, air contains 2,300 of oxygen for 7,700 of azote; in volume, 208 of the first for 792 of the second. The air, besides, contains from 4 to 6 10,000ths of carbonic acid in volume, whether it be taken at Paris or in the country. Ordinarily, it contains 4 10,000ths. Moreover, it contains a nearly equal quantity of the carburetted hydrogen gas, which is called marsh gas, and which stagnant waters disengage perpetually.

We do not speak of aqueous vapour, which is so variable: of oxide of ammonium and of nitric acid, which can only have a momentary existence in the air, because of their solubility in water.

The air, then, is constituted of a mixture of oxygen, azote, carbonic acid, and marsh gas.

The carbonic acid in it varies, and indeed, greatly, since the differences in it extend almost from the simple to the double, from 4 to 6 10,000ths. May not this be a proof that plants take from the air this carbonic acid, and that animals take back a part of it? In a word, may not this be a proof of that equilibrium of the elements of the air attributed to the inverse actions which animals and plants produce upon it?

It has, indeed, been long since remarked, that animals borrow from the air its oxygen, and give to it carbonic acid: plants in their turn, decompose this carbonic acid, in order to fix its carbon, and restore its oxygen to the air.

As animals breathe continually; as plants breathe under the solar influence only; as in winter the earth is stript, whilst in summer it

is covered with verdure, it has been supposed that the air must transfer all these influences into its constitution.

Carbonic acid should augment by night, and diminish by day. Oxygen, in its turn, should follow an inverse progress.

Carbonic acid should also follow the course of the seasons, and oxygen obey the same law.

All this is true, without doubt, and quite perceptible as to a portion of air limited and confined under a jar; but, in the mass of the atmosphere, all these local variations blend and disappear. Accumulated centuries are requisite in order effectually to put in action this balance of the two kingdoms, with regard to the composition of air; we are then, very far from those daily or yearly variations, which we had been apt to look upon as being as easy to observe as to foresee. With regard to oxygen, calculation shows that, exaggerating all the data, not less than 800,000 years would be required for the animals living on the surface of the earth to consume it entirely.

Consequently, if we suppose that an analysis of the air had been made in 1800, and that during the entire century plants had ceased to perform their functions on the surface of the whole globe, the animals at the same time all continuing to live, the analysts in 1900 would find the oxygen of the air diminished by 1-8000th of its weight: a quantity which is beyond the reach of our most delicate methods of observation, and which, assuredly, would have no influence whatever on the life of animals or plants.

As to this, then, we cannot be deceived; the oxygen of the air is consumed by animals, who convert it into water and carbonic acid; it is restored by plants, which decompose these two bodies.

But nature has arranged everything so that the store of air should be such, with relation to the consumption of animals, that the want of the intervention of plants for the purification of the air should not be felt until centuries have elapsed.

The air which surrounds us weighs as much as 581,000 cubic kilometres of copper; its oxygen weighs as much as 134,000 of these same cubes. Supposing the earth peopled with a thousand millions of men, and estimating the animal population at a quantity equivalent to three thousand millions of men, we should find that these quantities united consume in a century only a weight of oxygen equal to 15 or 16 cubic kilometres of copper, whilst the air contains 134,000 of it. It would require 10,000 years for all these men to produce a perceptible effect upon the eudiometer of Volta, even supposing vegetable life annihilated during all this time.

In regard to the permanence of the composition of air, we may say with all confidence, that the proportion of oxygen which it contains is secured for many centuries, even reckoning for nothing the influence of vegetables, and that nevertheless, these restore oxygen to it incessantly, in quantity at least equal to that it loses, and perhaps more; for vegetables live just as much at the expense of the carbo-

nic acid furnished by volcanoes, as at the expense of the carbonic acid furnished by animals themselves. It is not then for the purpose of purifying the air that these breathe, that vegetables are especially necessary to animals; it is, above all, to furnish them incessantly with organic matter quite ready for assimilation: organic matter which they may turn to their advantage.

There is, therefore, a service necessary, without doubt, but so remote, that it can scarcely be recognised, which vegetables render us, in purifying the air which we consume. There is another service, so immediate, that if, during a single year, it were to fail us, the earth would be depopulated; it is that which these same vegetables render us by preparing our nutriment, and that of all the animal kingdom. In this, especially, is found the chain that binds together the two kingdoms. Annihilate plants, and the animals all perish of a dreadful famine; organic nature itself entirely disappears with them in a few seasons.

We have, however, said that the carbonic acid of the air varies from 4 to 6 10,000ths. These variations are very frequent, and very easy to observe. Is not this a phenomenon reproaching the influence of animals who introduce this acid into the air, and that of vegetables which deprive it of it?

No; this phenomenon, you are aware, is a simple meteorological phenomenon. It is with carbonic acid as with aqueous vapour, which forms on the surface of the sea, to become condensed elsewhere, fall again in rain, and be reproduced under the form of vapour. This water, which is condensed and falls, dissolves, and carries with it carbonic acid; this water, which evaporates, yields up the same gas to the air.

A great meteorological interest would attach to the observation of the variations of the hygrometer, and those of the seasons, or of the state of the sky with the variations of the carbonic acid of the air; but hitherto all tends to show that these rapid variations constitute a simple meteorological event, and not, as has been thought, a physiological event, which, singly considered, would infallibly produce variations infinitely slower than those which are, in fact, observed as much in towns as in the country itself.

Thus the air is an immense reservoir, whence plants may for a long time derive all the carbonic acid necessary for their wants; where animals, during a much longer time, will find all the oxygen that they can consume. It is also from the atmosphere that plants derive their azote, whether directly or indirectly: it is there that animals finally restore it.

The atmosphere is, therefore, a mixture which unceasingly receives and supplies oxygen, azote, or carbonic acid, by means of a thousand exchanges, of which it is now easy to form a just idea, and the details of which a rapid analysis will now enable us to appreciate.

(To be continued).

Formation and Dispersion of a Thunder Shower; Parhelia, and Meteorological Register. By Willis Gaylord.*

In looking over my meteorological notes for 1839, under date of August 21st, I observed the following:—"Witnessed the formation and dispersion of a thunder shower, attended with some remarkable phenomena;" and as the formation and action of clouds and storms is always an object of interest, I have thought a description of the one alluded to might not be altogether without its claims to notice.

The wind on the 21st and for two days previous had been southwardly, most of the time S. E. The 20th was one of the warmest days of the season, the thermometer at 2 o'clock being at 90°, and on the 21st the mercury at 9 o'clock was 73°, and at 2 o'clock at 80°. Although the lower current of air was south, the upper did not seem to follow the same course, but was more from S. of W. This was shown by the course of some electric clouds on the 20th, and of one on the forenoon of the 21st. A little after 2 o'clock on the 21st I observed a large mass of cumulus in the S. E., not at a great distance, and with little apparent elevation. An electric cloud which was passing lay low in the horizon at the S., but between the two there was no connection; the mass of cumulus was completely isolated, a line of blue sky being distinctly visible between the two; nor was there any appearance of stratus, or the cirri, which invariably accompanies an electric cloud. There was no perceptible wind from any quarter.

I was in my garden some ten or twenty minutes after making the above observations, and not far from 3 o'clock, when my attention was arrested by a heavy roaring in the direction of the cloud, like that which accompanies a fall of hail or violent wind, and looking at the cloud, I perceived that a mass of cirri was streaming from the summit of the mass, and stretching upwards and N. E. from its highest point. There was little appearance of stratus at this time, and not the slightest indication could be discovered that rain or hail was falling from the cloud. I carefully examined the cloud to detect any motion which might exist in it, but not the least movement was perceptible, except that in a few minutes the stratus began to form rapidly at the base of the cloud, and a visible prolongation and elevation of the cirri was taking place. In a short time rain could be discovered precipitated from the cloud, and the roaring noise continued without interruption, exhibiting a singular contrast to the quiet and immovable state of the cloud. At this time the cloud was about three miles distant, and the angle of elevation shows its height to have been about six hundred feet.

The general movement of the cloud, it was soon apparent, was to the N. W., and in about twenty minutes after the first indications of a shower, I was driven within doors by a fall of the largest

* From Silliman's *American Journal of Science.*

drops of rain I think I have ever seen. They were not numerous, but in falling seemed as large as cherries, and dashed upon the earth with the seeming force of a hailstone. No hail was observed by me, but the size of the drops excited general notice. A heavy shower of perhaps ten minutes followed these drops, but during the whole, though the roaring noise continued unabated, not the slightest wind in any direction could be felt, but the water poured down perpendicularly like a cataract. This was particularly observable when the shower had passed so that the line of fall was about one hundred rods to the west of us. While it was a blue sky over head, at that distance from us, for ten minutes the water was pouring down in a vast sheet, and one mile west of us more water fell than during any other shower of the season. Before the shower had become perpendicular to us, or perhaps twenty minutes after the first rain fell from the cloud, thunder was heard in it; and after it passed, several electric explosions occurred. About five miles to the N. W. it ceased to rain, and the cloud rapidly melted away; and in two hours from its commencement nothing was to be seen of it except the train of cirri, resembling a streak of white smoke high up the sky.

But the most singular part of this electric shower remains to be noticed. During the time of its passage, on the eastern margin of the cloud, about two miles northeast of us, little rain fell, but *hail and snow* were both precipitated from the atmosphere. On the west side of the cloud the thermometer was but little affected, not more than is usually the case in summer showers; on the eastern or northeastern side, the cold was perceptible, and the thermometer fell rapidly; but in neither case was their any apparent atmospheric movement to account for such a change. I may remark here, that while the cloud remained stationary just west of us, it was rapidly extended to the south more than a mile, giving a heavy fall of rain to its extreme limit.

I have no particular theory to support or promulgate in giving you the foregoing. One of two things is perfectly clear from the facts as observed by me. The first is, that there was no *visible* rotary movement in the cloud at any time; and the second is, that there was no rush of *surface air* to the cloud, which would seem necessary had the noise been occasioned by an internal or central whirl. I have never known a thunder shower in which such a perfect stillness of the whole atmosphere was observable as during the continuance of this. Still some such movement as this seems necessary to account, not only for the noise that attended the cloud, but also for the rapid elongation of the cirri, and the formation of the hail and snow. It would seem that by some ascending movement, the vapour of the cloud and the drops of rain were brought in contact with air below the freezing point; and the large drops of water that fell on the western line of the shower must have been the result of a rapid condensation of vapour by contact with air

slightly above that point. Is it not possible that, owing to the different directions of the upper and lower strata of air, a rotary or upward movement may have been produced, drawing into it and elevating the vapour of the upper masses of cloud, the space thus created being filled by more elevated and colder masses, the motion of which to this point would account for the roar, as well as show how the condensation or congelation that took place might have been produced? In this case the lower air might have remained, as it certainly did, perfectly quiescent, while the upper air was in the greatest agitation.

Parhelia.

January 1st was the coldest day we have thus far had this year at this place. The thermometer at 7 o'clock was at — 12°, at 9 o'clock — 10°, and at 2 o'clock — 4°. It had snowed constantly for about three days, and the average depth was not less than three feet, the wind from the north. On the 2nd, the wind was N. W., the thermometer at nine o'clock at zero, and at 2 o'clock 9° above. At sundown it sunk to 0. A dense haze seemed to hang like a curtain in the west, and a little before sundown, brilliant parhelia were seen, resembling two mock suns. Their appearance when about five degrees above the horizon was somewhat like the following:—

Fig. 1.

On Thursday the 16th of January, a day which was generally noted as one of the coldest ever known in this country (the thermometer being at Albany — 26°, at Schoharie 36°, at Utica — 21°, at Syracuse — 14°, at this place — 10°, and at Franconia in New Hampshire 41°), occurred another beautiful spectacle of this kind. When the sun was about a quarter of an hour high the appearance was as below.

Fig. 2.

The colours of the parhelia in this case rivalled the most splendid appearance of the rainbow, and retained them until the sun sunk below the horizon. At that time, what may be called the upper limbs of the parhelia seemed to stand like beautiful columns of coloured light on the base of the horizon.

The next morning, the thermometer being at —6°, the moon, which set at about 6 o'clock, for more than an hour before going down, exhibited the most perfect and splendid paraselene ever witnessed in this place. The appearance was seen in Fig. 3; and was destroyed only by the moon's passing behind a cloud a few moments before setting.

Fig. 3.

To what cause these meteorological phenomena are usually attributed I know not, unless to atmospheric vapour; but in all these cases they seem fairly to owe their origin to the state of the air consequent on the intense cold. The air in such a state of cold is filled with minute crystals of frost, and the reflection from these is perhaps sufficient to account for the general appearance. But the difference in the figure of these parhelia would seem to prove that this general cause must be subject to many modifications from other agents. Is this change of figure owing to the different forms which it is well known the crystals of snow assume at different times? The explanation I leave with you.

Meteorological Register.

Below I have prepared a table of the average temperature, the weather, winds, &c., for the years 1838 and 1839, as observed by me at this place. Otisco is about fifteen miles west of south from Syracuse, and at an elevation of eight or nine hundred feet above that place, on the Seneca branch of the Erie Canal.

Year.	Average Temperature		Weather, Days.				Winds and Course.							
	9, A. M.	2. P. M.	Clear	Cloudy	Rain.	Snow	N.	N. W.	W.	S. W.	S.	S. E.	E.	N. E.
1838	41	48	162	200	77	78	18	72	154	66	29	6	2	16
1839	42	51	184	154	[illegible]	55	19	66	135	62	42	26	28	10

The extreme range of the thermometer in 1838 was between — 8° on the last day of January, and 93° on the last day of July, giving 101°. The range for the year 1839 was — 8°, January 23rd, and 90° on the 30th of July, giving 99°. An instance of those

sudden changes which occur in our climate, took place on the 19th of October, 1839, when the wind, which during the fore part of the day had been S. W., at half-past 2 o'clock suddenly veered to N. W., and the thermometer fell from 65° to 24° in three and a half hours; a difference of 41 degrees.

I have for several years noticed the fact, that whatever may be the direction or course of the lower strata of clouds, that of the cirri, or highest of all clouds, is almost invariably from west to east. It is nothing uncommon to see the lower clouds drifting in heavy masses, and with a strong wind to the N. or N. E., while far above them, the streamers of the cirri are floating undisturbed towards the S. E. or E. Indeed it is very rarely observed that cirri take any other course, and it may fairly be inferred they never do, until by greater condensation they approach the nature of cumulus, and sink into the action and influence of the lower currents. From their observations on these clouds on the Cordilleras of South America and Mexico, Humboldt and Boussingault have inferred, and I think with good reason, that in the upper regions of the atmosphere there is a current constantly flowing from west to east, an inference which, if admitted, assists materially in developing the theory of storms, sudden changes of temperature, &c.

A glance at that part of the table showing the course of the wind will explain the fact noticed by Darby and others, that the mass of trees growing on the eastern shore of the great lakes have a sensible inclination to the east, and that in all cases where the hemlock occurs, the long, flexible, terminal twig of that tree has the same uniform declination from the perpendicular, and in the same direction, a little north of east. The same thing may be observed of orchards, in which probably nine tenths of the trees in exposed situations have a similar inclination. In the first year, two hundred and twenty days of the three hundred and sixty-five the winds were from the W. and S. W., and in the last one hundred and ninety-seven. The remaining days the winds were so equally divided as not to counteract this influence in the least, and consequently the winds from that quarter overpower all others.

The average temperature of the years 1838, 1839, and 1840, for the months of January and February, is given below.

	January.	February.
1838	26°	9°
1839	20°	24°
1840	14°	26°

Time of observation, 9 o'clock, A. M.

Thus, it seems January of this year averages 12° colder than 1838, and February of this year 17° warmer than that year.

The month of April with us has been remarkable for its extreme and rapid fluctuations. The warmest day recorded of any April here, was on the 25th. On that day the thermometer in the shade

at 2 P.M., was at 85°, and at 3 P.M., 86; on the 27th at 6 A.M., it stood at 28°; being a change of 58° in 37 hours. On the 18th at noon the thermometer was at 80°; on the 19th at sunrise, it was at 29°; being a change of 51° in 18 hours. The range of the thermometer from Jan. 1st, when it was — 14°, to April 25th, when it was +86°, is 100°; a difference rarely equalled in our changeable climate.

A Description of a Halo or Corona of great Splendour, observed at Greensburgh, Westmoreland County, Pa. By ALFRED T. KING, M. D.*

IF you consider the subjoined description of one of those meteorological phenomena, usually denominated by philosophers coronas or halos, which was observed in this town about eleven o'clock A.M., on the 28th of August last, and which excited considerable interest among the intelligent portion of the community, and apprehension and alarm in the minds of the uninformed, worthy of a place in your excellent journal, it is much at your service.

This phenomenon consisted of from three to five circular belts or zones of light, one of which emulated, in appearance, the splendor and magnificence of the most gorgeous rainbow. The arrangement of these rings are somewhat singular; the first, or inner one, which had the sun for its centre, was *truly* brilliant, exhibiting all the prismatic hues of the rainbow, the colours of which were so dazzling that the unprotected eye could scarcely rest upon it a moment. This, I presume, was occasioned by the sun being near the meridian, and consequently many of his rays would impinge upon the halo, without passing through the mass of vapour, to the existence of which

* From Silliman's *American Journal of Science.*

I attributed the formation of the halo. The outer circles, however, one only of which appeared to be perfect, were composed of pure white light, and had for their centres the circumferences, or a point near it, of the inner ring. Consequently, these circumferences, if all the circles had been perfect, would necessarily have passed through the apparent situation of the sun. I mentioned, however, that only one of these rings was perfect, the others were concentric arcs of circles which crossed one another, as seen in the accompanying diagram.

In the centre of the inner circle and bounded by it, a bluish mass of dense vapour was perceptible, which gave to the whole an embossed appearance, and added much to the beauty and brilliancy of the scene. Around and within the exterior circles there were also perceptible masses of vapour, though obviously much less dense than the mass which was nearer the sun. With the exception of these masses of vapour, and a large *cumulus* which lay to the south of us, and here and there a few scattered *cirri*, the sky was cloudless and the atmosphere calm and serene. The mercury in the thermometer stood at 86°. The weather continued thus for thirty-six hours, when we had a smart fall of rain, and a descent of the mercury in the thermometer to 36°, at which point or near this, it has remained until about three days since, when it rose to 66°.

Coronas and *parhelia* have frequently been observed and accurately and glowingly described by many scientific gentlemen, and various and conflicting opinions have been entertained respecting their causes; some attributing them to the peculiar state of the air consequent upon intense cold, while others, probably more correctly, attribute them to the refraction and reflection of the rays of light through masses of vapour which are formed in such aggregations as are not heavy enough to fall in the form of drops. Descartes remarks, that halos never appear when it rains. Coronas have frequently been observed around the moon, and even around Sirius and Jupiter, but, as far as my information extends, they have been but seldom variegated, even when they have encircled the sun.

I know not to what cause this phenomenon can be attributed, unless it be to the refraction and reflection of the sun's rays through the masses of vapour. Doubtless the first circle was thus formed, and if we suppose the rays of light from the circumference of this circle to be again refracted and reflected through another mass of vapour, an outer ring would evidently result. Again, if we suppose the same to take place from another point of this circle, a second ring would be formed which would cross the other in some point of its circumference, and in like manner, I presume, any number of rings may be formed. I offer this explanation, however, with much diffidence.

On the Variable. Action of the Electric Column. *By* Mr. J. A. De Luc.*

(Continued from page 243).

Sir,— Your number of June last contains a paper of Mr. Ronalds's, which could not but interest me, as it relates to the variable action of the electric column. This variableness depends on many causes, with respect to one of which we do not agree, and it will be the object of this paper.

I have found in my experiments, that an increase of moisture did increase the action of the column. Mr. Ronalds is of a different opinion; but as he describes the experiments from which he concludes that moisture has a very little effect on that action, it will be easy for me to show that this disagreement between us results from a mistake on his part, and that his experiments, though very ingenious, according to his idea of the subject, are quite dissimilar to those whence I had derived my conclusion.

One single circumstance will show that Mr. Ronalds did not understand my meaning. I could not refer but to the column which I had described, of which an essential circumstance is, that it is in communication with the outside air, and thus influenced by its degree of moisture; whereas Mr. Ronalds's experiments have been made with one of Mr. Singer's columns of 1000 groups inclosed in a glass tube: that tube therefore precluding the action of the external air, the inclosed column cannot be affected by its changing degree of moisture. I do not doubt that when Mr. Ronalds shall consider that difference between our experiments, he will judge that the results which he relates in his paper, though interesting in themselves, cannot be opposed to mine: but I must come to particulars, because they will contribute to a further explanation of this new physical apparatus.

The action of Mr. Ronalds's column was indicated by the number, in a given time, of the strikings of the gold leaves in an electrometer: in his second experiment made under a glass receiver, the strikings having been five in the beginning, while the hygrometer was 41°, and the thermometer 53½, the strikings were reduced to three, when the hygrometer ascended to 41½, and the thermometer descended to 53°: which experiment appears to favour Mr. Ronald's idea, that the increase of moisture rather lessens than increases the action of the column. But in this experiment the column being inclosed in the glass tube, the increase of moisture took place on the surface of the latter, which produced an increase of conductive faculty for the electric fluid on that surface, which lessening the difference of electric state of its extremities, diminished consequently its effect on the electrometer.

The third experiment, in which the room was gradually heated, shows a case in which I had partly made a mistake, pointed out to

* From Tilloch's *Philosophical Magazine.*

me by Mr. Singer. In one of my experiments, when the sun-rays fell on my column the number of strikings sensibly increased, and returned gradually to their former number when the sun-rays ceased to fall on it: this effect I attributed entirely to the action of the sun-rays themselves; but Mr. Singer conceived that it was only the effect of heat thus increased in the column. I considered this explanation as deserving to be submitted to direct experiments; I have since long observed the column under this point of view, and found that the increase of heat had a great influence to increase the number of strikings. But this is not an effect so simple as I thought it at first; for, with the same increase of heat, I have found great differences in the increase of the correspondent number of strikings; which circumstance I continue to think must be owing to the difference in the electrical state of the ambient air; and thus the column is, as I had found it in my first observations, an aërial electroscope, which property Mr. Ronalds had also surmised. But too many causes interfere with that state of the ambient air, to distinguish that effect with certainty; and it will require a longer study of the variation of the effects by different circumstances, in order to assign the true influence of each of them.

Mr. Ronalds's experiment 7 is a confirmation of those which I had described in a paper published in the *Philosophical Transactions* of the Royal Society for 1791. These experiments were made in order to show the cause of a dissentiment between M. de Saussure and myself on the point of extreme moisture, which I had prescribed to be taken in water itself; the reason of which I stated in my first paper on the hygrometer, published in the *Philosophical Transactions* for 1774. The construction of M. de Saussure's hygrometer did not permit to dip it in water, because the index was in the lower part of the frame, and the hair of which it is formed could not be dipped in water without the whole instrument, its index and scale, being immersed into water: he therefore fixed its point of extreme moisture under a receiver inverted on a dish full of water.

Unluckily this circumstance did not permit M. de Saussure to discover the defect of the hair and of all the threads of fibrous substances, which is to relent successively their lengthening when moisture increases, and even at last to grow shorter, more or less according to their nature, when moisture approaches to its extreme; which circumstance excludes all the threads from a true hygrometer. But my hygrometer, consisting of a slip of whalebone cut across the fibres, is not subject to that defect; it lengthens up to the point of extreme moisture taken in water, and with it I made the same experiment under the receiver inverted on water; and thus I found the important law of hygrology, that the utmost quantity of evaporated water under such a receiver does not produce extreme moisture in the enclosed space when the degree of heat is sensibly above 32: and that it recedes from it in proportion as the heat increases. Mr. Ronalds has found the same effect in his experiment 7; for as long as the temperature was 55, which lasted a

long while, the hygrometer never attained a point higher than 93.

In the same experiment under the receiver inverted over water, Mr. Ronalds found the number of strikings gradually to decrease, from 4·5 in a minute even to no striking. I am not surprised at this effect, as it proceeds from the cause which I have already indicated; for the glass tube in which the column was inclosed being more and more covered with moisture, its surface was at last become such a conductor of the electric fluid from the positive to the negative end, that it became neutral.

The last experiments of Mr. Ronalds would have surely reconciled our opinions on this subject, had they been made on purpose for its investigation; being made with a column similar to mine, composed of 800 groups, supported between three glass pillars covered with sealing-wax; but these experiments were made for a different purpose. He placed an hygrometer, a thermometer, and an electrometer under a receiver inverted over mercury, in order to introduce successively an acid and an alkali, and to observe the degree of dryness they would produce: with the acid the hygrometer descended from 46½ to 29½ by the temperature 59½; having then removed the acid and substituted potash, in the course of a day it brought the hygrometer to 24.

These experiments, and the following, in which Mr. Ronalds made moisture increase under the receiver by introducing in it a moistened card, were made with the view of trying the effect of more or less moisture for increasing the action of the column, shown by the number of strikings of the gold leaf in the electrometer, in a given time, compared with the effects of the changes of heat: they certainly prove that the increase of heat accelerates the strikings, as Mr. Singer had supposed; but they show at the same time, what small quantity of evaporated water in a given space, produces in it extreme moisture, and that the increase of heat with the same quantity of water, tends to diminish moisture.

This I know by my own experiments, which are the object of my paper in vol. xxxiii of Mr. Nicholson's *Philosophical Journal*, giving an account of two series of experiments, agreeing with each other, by which I determine the number of grains of water which can remain in the state of vapour in the space of one cubic foot, by each degree of my hygrometer and of the thermometer. It may be seen in the table of the results, that seven grains of water evaporated in one cubic foot, by the temperature of 60, brought the hygrometer to 96·6 degrees, and that then the smallest diminution of heat caused a deposit of water on the sides of the vessel. This shows what small quantities of evaporated water act on the hygrometer in all its degrees; an important circumstance to be attended to in meteorological systems; a subject to which I shall return.

The above experiments made with the greatest care in a glass vessel, which was air-tight, show that such experiments made under a receiver are more difficult than is commonly imagined, and that they cannot be opposed to my experiments made in open air, which de-

monstrate that the action of the column is increased by the increase of moisture, and diminished by dryness. But a more direct proof of it is related in the same *Philosophical Journal* of Mr. Nicholson, for August, 1810, by some experiments which I have made with my late very ingenious friend Dr. Lind, for ascertaining immediately the influence of moisture on the action of the column.

In this experiment all the parts of a column were first separately laid on the hearth of a chimney before a great fire, so that the pieces of paper were almost singed. In that state we mounted the column, and it did not affect the gold leaf electrometer. We then dismounted again that column, and laid also the separate pieces on a table in my room, in which the hygrometer was about 40°. When they had thus remained one hour, we remounted the column, and it acted on the gold leaves as it did before the papers had been so thoroughly dried by a great heat.

This I think to be a direct fact proving that a certain degree of moisture in the column is indispensable for its conductive faculty, the source of all its effects. I have found in all my observations, that within certain limits an increase of moisture increases the action of the column; but, whereas the effect of the increase of heat is immediately perceived, as it easily penetrates the column, that of the increase of moisture is very slow, because, beginning at the edge of the papers on the outside, it very slowly propagates in the internal parts of the column; a circumstance of which Mr. Ronalds has not been aware, especially by his column being inclosed in a glass tube.

But the column being a very new apparatus, it requires some time and a greater number of observers to follow all the views it opens in the terrestial phenomena, especially in those of the atmosphere which constitute meteorology. This was my general conclusion in a paper published in vol. xxxiii of Mr. Nicholson's *Philosophical Journal*, under this title: "On Hygrology and Hygrometry, and their Connexion with the Phenomena observed in the Atmosphere." There I demonstrated that important fact, not only in meteorology, but in natural philosophy, that rain does not proceed from a quantity of aqueous vapour or moisture existing at any time in any portion of the atmosphere; that it must proceed from the decomposition of the atmospheric air itself, from which decomposition more or less complete result all the meteors, lightnings, thunder, hail, and other atmospheric appearances.

I have been thus particular in the examination of Mr. Ronalds's opinions, as he has shown much ingenuity in his experiments, and because, having had the opportunity of being personally acquainted with him, I agree with the judgment of Mr. Singer, in the opening of the same paper, that Mr. Ronald's "is an electrician of great promise, and whose scrupulous attention to the essentials of accurate experimental inquiry has afforded me pleasure to observe."

I am, sir, your most obedient servant,

J. A. De Luc.

Windsor, September, 1814.

Experiments on the Variable Action of the Electric Column. By Francis Ronalds, Esq., *Communicated by* Mr. Singer.*

Very soon after I had constructed a pendulum apparatus similar in principle to that of M. de Luc, described in vol. xxvii, page 161 of *Nicholson's Journal*, and had observed the variety of its action, I suspended an hygrometer and a thermometer near it, by which I found that when the latter rose but one degree, a difference of four to five vibrations in a minute took place, whilst no such effect occurred if the former advanced two or three degrees towards dryness unaccompanied by a corresponding rise of the thermometer. It was therefore evident that heat is the principal cause of the variable action of the electric column.

The following experiments were made with a gold-leaf electrometer, in order that they might be repeated and varied by any one who may not be inclined to construct the pendulum apparatus of M. de Luc's contrivance, or provide himself with one.

The negative end of a column consisting of 1000 groups of zinc and gold paper, enclosed in a glass tube with interposed discs of paper, as improved by Mr. Singer, was screwed upon one side of a brass tripod, an electrometer on another side, and an hygrometer and thermometer on the third side. A glass dish was placed under the tripod, and the whole apparatus arranged upon the plate of an air pump.

An observation was generally made every five minutes. In the third experiment the mean of several observations is set down instead of each for the sake of brevity. The first column gives the time; the second the degree of the hygrometer; the third that of the thermometer; and the fourth gives the number of strikings per minute of the gold leaves.

EXPERIMENT 1 IN OPEN AIR.

Time. h. m.	Hygrom.	Therm.	Strik.
3	41	53½	5
3 5	41½	idem	idem
3 10	idem	id.	id.
3 15	id.	id.	id.
3 45	41	54	id.
4	id.	53½	id.

EXPERIMENT 2 IN THE RECEIVER.

Time. h. m.	Hygrom.	Therm.	Strik.
4 5	41	53½	5
4 10	idem	idem	idem
4 15	id.	id.	id.
4 30	id.	id.	id.
6 15	41½	53	3

EXPERIMENT 3. ROOM GRADUALLY HEATED.

Time. h. m.	Hygrom.	Therm.	Strik.
6 30 to 6 45	41·12	53·75	3·75
6 50 to 7 0	40·33	55·33	5·66
7 5 to 7 15	39·5	57·5	6·5
7 20 to 7 40	38.28	58·8	7·4

* From Tilloch's *Philosophical Magazine*.

Time. h. m.		h. m.	Hygrom.	Therm.	Strike.
7 45	to	8 0	37·5	61·18	8
8 5	to	8 30	36·83	63·33	8·66
8 35	to	8 45	35·66	65·33	8·66
8 50	to	9 5	35·22	66·25	10·5
9 10	to	9 25	34·5	67·25	12·5
9 30	to	9 50	33·25	68·5	13·5
9 50			32	70	17
9 55	to	10	31·5	71	leaves stuck to the sides frequently.

EXPERIMENT 4. RECEIVER REMOVED.

Time. h.	m.	Hygrom.	Therm.	Strik.
0	20	40·5	52	2·5
	25	idem	idem	5·3
	30	id.	id.	idem
	35	id.	id.	id.
	40	id.	id.	id.
	45	id.	id.	id.

EXPERIMENT 5. SULPHURIC ACID PLACED IN THE DISH.

Time. h.	m.	Hygrom.	Therm.	Strik.
	55	37	52	2
1	0	31	id.	id.
1	5	29	id.	id.
1	10	25	52·5	2·5
1	15	21	id.	3
1	20	19	id.	2·5
1	25	18	id.	3
1	30	16	id.	id.
2	15	11	53	id.
2	30	10	53·25	3·5
2	45	9·5	53·5	3
3	0	9	54	3·5
3	15	8·5	id.	5
3	30	8	id.	4·5
3	45	id.	54·25	5
4	0	7·5	id.	id.
4	15	id.	id.	id.
4	30	id.	54·5	id.
4	45	7	id.	id.
5	55	6·5	54·25	4

EXPERIMENT 6. RECEIVER AND ACID REMOVED.

Time. h.	m.	Hygrom.	Therm.	Strik.
6	5	25	55·5	6
6	20	36	id.	id.
7	0	37	id.	id.

EXPERIMENT 7. WATER PLACED IN THE DISH, AND RECEIVER REPLACED.

Time. h.	m.	Hygrom.	Therm.	Strik.
7	15	39	55	4·5
7	20	43	54·5	id.
7	25	48	id.	id.
7	35	63	id.	3·5
7	40	66	id.	id.
7	45	69	id.	3
7	50	71	id.	id.
7	55	74	id.	2·5
8	0	75	id.	id.
8	5	76·5	id.	2
8	10	78	55	id.
8	15	79·5	id.	id.
8	20	80·5	55·5	3
8	25	81	54·25	2·5
8	30	82	id.	id.
8	35	82·5	id.	id.
8	40	83	id.	2
8	45	83·5	id.	id.
8	50	84	id.	id.
8	55	84·5	id.	1·5
9	0	85	id.	id.
9	5	85·5	id.	id.
9	10	86	id.	id.
9	15	86·5	id.	1
9	20	87	id.	0·5

Time. h.	m.	Hygrom.	Therm.	Strike.	Time. h.	m.	Hygrom.	Therm.	Strike.
9	25	87·5	id.	0·3	10	10	91	id.	0·7 in.div
9	30	88	id.	0·28	10	20	91·5	id.	0·6 do.
9	35	88·5	id.	id.	10	30	92	id.	id.
9	40	89	id.	0·25	10	40	92·5	55	id.
9	45	89·5	id.	0	10	50	93	id.	id.
9	55	90	id.	0	11	55	95	53	id.
10	0	90·5	55·25	1 in.div					

Experiment 1. Was destined to try whether at equal degrees of heat and moisture in the open air an uniform action prevailed, and the result shows that it was nearly so.

Exp. 2. Shows that the same uniformity continued in the receiver, which was not air tight.

Exp. 2. Shows that when the air of the receiver was gradually heated from 53 to 70, and by this means the hygrometer made to advance $9\frac{1}{2}$ degrees towards dryness, the electrometer increased its strikings progressively from 3·75 to 17 times per minute.

Exp. 4. Proves that the column had not undergone a permanent change by experiment 3.

Exp. 5. Shows that when the air of the receiver was dried by means of sulphuric acid from 37 to $6\frac{1}{2}$, giving for the whole diminution of moisture $30\frac{1}{2}$ degrees, no increased intensity took place.

Exp. 6. Proves that the column had not been permanently affected by the last experiment.

Exp. 7. Shows that when the air was moistened 56 degrees, the intensity was diminished from $4\frac{1}{2}$ strikings per minute to a divergence of $\frac{6}{10}$ inch.

It would be unnecessary and tedious to detail some other experiments which were made with a different view, but which conspired with the above to indicate that heat is the principal cause of the phenomenon, and that a moist atmosphere produces exactly the same effect on the glass tube of the column that it does on the insulators in all other electrical experiments.

It now therefore seems a matter of some interest to resolve several queries relative to the mode in which heat acts: for instance, does it promote the electromotive powers of the metals? Does it produce the effect by giving motion to the combined moisture of the interposed discs of paper, or by rendering those discs more or less conducting? Is there any analogy between the column and the tourmalin? The justly celebrated inventor of this modification of the Voltaic pile, having first observed that equal degrees of heat and moisture were not accompanied at different times with a corresponding intensity of action, conceived that this difference was occasioned by changes in the electrical state of the ambient air, for which reason he gave it the name of Aërial Electroscope, and I think the evidence of our present experience preponderates in favour of the conjecture.

Beccaria, Read, and others have shown that the state of moisture of the higher regions of the atmosphere is intimately connected with that of its electricity: a height of only six or eight feet above the surface of the earth is even sometimes sufficient for collecting powerful electric signs in an open situation. I have by means of a long wire, insulated on Mr. Singer's plan, when a copious dew was falling, collected pungent sparks when the height of the wire did not exceed five feet above the surface of the earth in any part of it. Hence it is concluded, that we cannot measure the electricity of the higher strata of the atmosphere until that of the lower, which may be excited by the evaporation or condensation of moisture, or by position, has been first measured. Now, if this electricity of the lower strata could be proved to exist in a very small degree in closer situations, there would be no reason why it should not influence the column.

Mr. Read has, in my opinion, done much towards this by aid of the doubler.

The signs which the doubler produced were certainly strongly analogous to those of his rod, and there are several other experiments which appear to me to prove that what is called the adhesive electricity of such like instruments is occasionally no other than that of the ambient air, which the peculiar structure is calculated to collect and display.

P.S.—Since writing the above, I have shown the tables to Mr. Singer, and was glad to find that he not only coincided in the conclusions drawn from them, but also in the idea that the electric state of the ambient air may be sufficiently powerful to influence the column: but I wished to ascertain the effect of heat upon a column not inclosed in a tube, as it might be objected that the air immediately in contact with the pile I used, and the inner surface, was not dried by the acid, although the hygrometric equilibrium might have been varied by heat. I therefore borrowed from Mr. Singer a pile consisting of 800 groups of zinc, silver, and paper, supported between three pillars of glass covered with sealing-wax, and placed in a very large receiver, together with an hygrometer, thermometer, and electrometer: the receiver was now placed over mercury.

The divergenee of the gold leaves was estimated as nearly as I could guess by the eye, and the following results were obtained:—

	Time. h.	m.		Hygrom.	Therm.	Electrom.
June 2nd.	11	40	A.M.	45	56½	divergence 1·5 inch
	3	0	—	42	76½	struck 3 times per minute
	10	0	—	45	64	1·2 — per minute
June 3rd.	1	0	P.M.	44	58	divergence 1·5 inch
			Receiver taken off.			
	3	30	—	46½	58	idem
	3	37	receiver placed and acid introduced			
	4	37	—	32½	59½	idem

	Time. h. m.		Hygrom.	Therm.	Electrom.	
	6 0	—	29½	59½	idem	
	6 30	acid removed and potash substituted				
	8 0	—	36	59	idem	
June 4th.	9 30	A.M.	32	56	divergence	1·25 inch
June 5th.	3 30	P.M.	30	53		1·1
	8	—	29½	54	idem	
	10	—	24	80	divergence	1·5
			Potash removed.			
June 6th.	11 0	A.M.	35	53	idem	
June 7th.	0 0	—	a moistened card placed in receiver.			
June 8th.	11 30	—	40	50	divergence	1·25 inch
	2 0	—	40	61		1·5
	3 30	—	37½	77	struck	1 per minute
	3 45	—	37½	80		3
	4 45	—	39	76		2·5
	5 45	—	40	71		1·5
June 9th.	9	another moistened card placed in receiver.				
June 10th.	6 0	P.M.	42	60	divergence	1·25 inch
	6 40	—	40	69		1·5
	6 50	—	39½	73	struck	2 per minute
	7 25	—	39	77		3
	8 0	—	39½	80		4·5

There are several circumstances deserving observation in these experiments, but one in particular: viz., that on the 5th of June, when the air had been dried by a long continued action of the alkali, the power of the column was not increased by a rise of temperature in its usual degree. Possibly the discs of paper had been also deprived of a part of the moisture which appears necessary to the action of the column.

Hammersmith, June 10, 1814.

A Method of Measuring the Force of an Electrical Battery during the time of its being Charged. By Lieut.-Colonel HALDANE.*

LET the battery be insulated, and at a small distance from it place an uninsulated electrical jar; also near to the jar place one of Mr. Cuthbertson's electrometers.

The electrometer being adjusted according to the degree of force which is intended to be employed as a measure of force to be communicated to the battery, connect the electrometer with the jar; make a metallic communication between the interior side of the jar and the exterior side of the battery, and connect the interior side of the battery with the conductor of an electrical machine.

Then, by the operation of the electrical machine, the battery receives a quantity of the electrical fluid, and becomes charged. The fluid which departs from the exterior side of the battery, is re-

* From *Nicholson's Journal.*

ceived by the electrical jar, which also becomes charged; but this jar, being connected with the electrometer, explodes as soon as it acquires a force sufficient to put the electrometer into motion.

Now, the quantity of the electrical fluid which is received by this jar between each of the explosions, is a measure of the quantity of the fluid in the battery; and the number of explosions or discharges of this jar shews the number of measures which the battery contains, and consequently the force which it is capable of exerting when discharged.

Demonstration.

The electrometer remaining under the same adjustment will require the same force to put it in motion: this force results from the quantity of electrical fluid received by the jar; and since it is admitted that when effects are the same, the causes of them must be equal, it is evident that the quantity of electrical fluid contained in the jar at the time of each explosion is the same.

It is also obvious that the sum of all these equal quantities of the electrical fluid which was contained in the jar at the time of each explosion, is equal to the whole quantity contained in the battery; for the battery being insulated the jar received all the electrical fluid which departed from the exterior side of the battery; and that quantity is said (in the theory of Dr. Franklin) to be equal to the quantity in the interior of the battery.

Therefore it is manifest that the number of explosions or discharges of the electrical jar, is the number of equal measures of the electrical fluid which the battery contains.

But without putting too much confidence in any philosophical theories, the effects of this operation may be more satisfactorily shown by the following experiments:—

Experiments.

A piece of iron wire, about 0.045 inches in diameter, and about two inches in length, was placed in the circuit through which the discharge of a small electrical battery, which contains about six feet superficial of coated glass, was to pass.

The electrical jar employed as the measure of the charge of the battery contained about ninety square inches; and the adjustment of the electrometer was varied in each set of experiments, by changing the weight applied to the balance, and also the distance of the discharging balls.

Experiment I.—The electrometer being adjusted with its least weights, the discharging balls placed at the distance of one inch, and other parts of the apparatus arranged as before described, the electrical machine was put in motion, the battery and also the jar began to receive a charge, as was shewn by the repulsion of a pith-ball on a graduated quadrant placed upon the electrometer.

1. After the first explosion of the electrical jar, that is, after the battery had received one measure of the electrical fluid, a discharging rod was applied to complete the circuit in which the iron wire was

placed; but, upon the discharge of the battery, no change of appearance was visible in the wire.

2. The operation of the electrical machine being continued, the discharging rod, after two explosions of the jar, that is, after the battery had received two measures, was applied as before; but, upon the discharge of the battery, no change appeared on the wire.

3. The battery was then charged with three measures; and upon discharging it as before, luminous particles of the wire were thrown off.

4. The battery, having received four measures, the wire, upon the discharge, exhibited nearly the same appearance as before.

5. The battery, having received five measures, was discharged; the wire was red-hot, and separated.

6. The battery, having received six measures, was discharged; the wire was dispersed in red-hot globules.

The battery, upon receiving between nine and ten measures, made a spontaneous discharge.

Exp. II.—In the second set of experiments, the apparatus was arranged as before, and the electrometer adjusted with the same weight, but the discharging balls were placed at the distance of two inches. The results were, upon the discharging of the battery, after having received

Measures.	
1 ...	No alteration in the wire.
2 ...	Luminous particles thrown off.
3 ...	The same, with smoke.
4 ...	Red-hot, and separated.
5 ...	Dispersed in red-hot globules.
Between 7 and 8 ...	A spontaneous discharge of the battery.

Exp. III.—The apparatus being arranged as before, the electrometer was adjusted with its greatest weight, and the discharging balls placed at the distance of one inch; the results were, upon discharging the battery, after having received

Measures.	
1 ...	No alteration in the wire.
2 ...	Luminous particles thrown off.
3 ...	The same.
4 ...	Red-hot, and separated.
5 and 6 ...	Red-hot, and dispersed in globules.
Between 8 and 9 ...	A spontaneous discharge.

Exp. IV.—The electrometer being adjusted with the greatest weight, the discharging balls were placed at the distance of two inches. The results were:

Measures.	
1 ...	Luminous particles thrown off.
2 ...	The same, with smoke.
3 ...	Red-hot.
4 ...	The wire was dispersed in red-hot globules.
Between and 7 ...	A spontaneous discharge of the battery.

Exp. V.—The apparatus remained as in the fourth experiment, with the addition of another battery, containing twelve feet of superficial coated glass, and the iron wire placed in the circuit of the discharge of this battery was about 0.08 inch in diameter, and two inches in length. The results were:

Measures.	
1 ...	No alteration in the wire.
4 ...	Luminous particles thrown off.
6 ...	The same, with smoke.
8 ...	Red-hot, and separated.
10 ...	Dispersed in red-hot globules.
Between 15 and 16 ...	A spontaneous discharge.

HENRY HALDANE.

Harley-street, May 27, 1797.

The electrical machine employed in these experiments has a glass cylinder of nearly 18 inches in diameter. It was constructed by Mr. Nairne, and is a most powerful instrument, particularly in exhibiting all the phenomena in which the negative, or electricity of the rubber is concerned.

An Inquiry into the Cause and Nature of Galvanism. *By* Mr. James Milne.

SECTION I.

From the great difficulties attending the theories of galvanism, by Davy, G. Lusac, Volta, and all others who are under the necessity of calling to their aid the assistance of attraction and repulsion, *two occult qualities* of matter, which may be said to have never been proved to exist, and of which we can form no idea of the manner in which they operate, I have been led to enquire whether a more simple and obvious property in bodies would not account for the whole phenomena, without having to call in the aid and operation of these unknown qualities of attraction and repulsion: and from the observations which I have been able to make in carefully reading over the whole experiments detailed in encyclopædias and other works, I am of opinion that the theory which I have to propose will sufficiently account for all the actions exhibited by that interesting element, without the aid of attraction or repulsion.*

Let it be supposed that the galvanic fluid is one of a most subtle and elastic kind, like heat, of which it is supposed to be a modification; that it is of universal extent, and pervades all nature; that it is a compressing elastic fluid, and has a tendency from its own pressure, like all elastic fluids, to be forced into the pores of all other bodies, whether elastic, liquid, or solid, in which there is space for it to enter, and which, from their open nature they can no more pre-

* The essay was written in December, 1833, and the author has had several corroborating proofs of the truth of the theory since that period.

vent its intrusion into them than sandstone can prevent the intrusion of water when immersed in it.

From the universal extent of this fluid it is evident that every substance or body which is porous must be full of it, and that all are surrounded with it; that wherever the balance or equilibrium is disturbed by any means, it will have a tendency to regain its former state; and with all the celerity which the nature of the fluid is calculated to produce. This fluid and electricity is supposed to be the same; but its effects are differently modified by the means taken to accumulate and discharge it: that the sun, and planets, and stars are surrounded with it; and that its extent is commensurate with the universe.

From the different texture of substances it must be evident that some will admit it much more freely, and part with it much more readily than others; just as some kinds of stone and sand imbibe and part with water much more readily than others. In considering these substances it must be recollected that the porous nature of many bodies admit water and air, and other gases, and that the galvanic fluid will occupy the pores in the air and water that are in these substances, and such other pores as may be impervious to the water, air, &c.

It is evident the different bodies must have different capacities for holding the galvanic fluid; some will hold double, treble, or more of it than others. It is found that metals admit and part with galvanic electricity readily, or that they are good conductors of it. What should be the cause of this property is not very obvious. If those are the best conductors which are most easily oxidable, would not this indicate that it is on account of the greater quantity of galvanic electricity they contain? But be this as it may, it does not affect the theory here proposed, for it will not be doubted or denied that the different substances have different capacities for galvanic electricity, in the same way as they are allowed to have for caloric. Admitting then that the same kinds of matter, and some kinds of metals, have the property of transmitting, or what is the same, of admitting and parting with galvanic electricity quicker than others, and that the fluid is universal; I shall attempt to describe how all the different galvanic phenomena may be accounted for by the known laws of matter.

In the galvanic trough the zinc plate is a better conductor of galvanic electricity than the copper one, that is, it admits and parts with it quicker. The diluted acids also are supposed to have their full share of galvanic electricity, and the plates and acids are so arranged that the zinc plate being of greater capacity for the galvanic fluid than the acid; therefore, a part of the galvanic electricity is taken from the acid and absorbed by the zinc in each of the divisions, by which the zinc is so overcharged as to communicate part to the copper. The copper becomes thereby over charged, and again communicates it to the acid in the next cell; so that a continual circulation of the galvanic electricity will go on till it accumulates at the right hand

acid, so as to admit no more from the copper, and the copper to admit no more from the zinc, and a state of equilibrium be induced. But if now a wire is used to connect the ends of the trough, the superabundant fluid at the right hand cell of acid will flow to the acid at the left hand cell along the wire; then it will again be communicated to the zinc, the copper, and the acid in each division, and form a circular stream till the acid or plates get oxidized, when it will cease to flow.

To make the operation more plain, suppose the zinc and copper plates soldered together, and put into the trough before the acid is put in, and that these plates are each as full as their capacities would permit, and in a state of equilibrium with respect to each other; but that the zinc admitted and parted with the electricity quicker than the copper, the acid being then poured into the trough between the plates, and as it does not hold the galvanic fluid in the same state of equilibrium with the plates of zinc or copper, it either communicates some of its electricity to the plates or admits it from them. Suppose that it possesses a surplus, and communicates it to the plates, each of the plates will admit a quantity proportioned to their capacities; but they may have different capacities as well as different powers for admitting and parting with it quickly; and we are here to suppose that the zinc admits it much quicker than the copper, and that, consequently, the zinc is saturated with it first or soonest, and is transmitting it to the copper before the copper has received all that it could admit from the acid, and that this has been going forward in all the divisions. The consequence would be that the acid would be deprived of a great quantity of its electricity in each trough, or division; and that the zincs being saturated before the copper, they would be giving the electric fluid to the copper, at the same time that the copper would be receiving it on the other side from the acid; but before the copper is in equilibrio with the acid and the zinc, the acid having parted with the galvanic fluid to both the zinc and copper, is reduced to the equilibrium of the copper, before the copper is in equilibrium with the zinc. The zinc, therefore, continues to pour it into the copper at the same time it is continuing to take it from the acid: the acid is by this means robbed of its galvanic fluid, and its equilibrium is also prevented; and the copper begins to give back to the acid, the acid to give to the zinc, and the zinc to the copper in the next cell, and so on till the whole would come to an equilibrio by the right hand acid, and the zinc having imparted as much to the copper as to put them in equilibrio, and the others in the same way. But suppose a wire or other conductor now to connect the extreme cells of the trough, by which the circuit would be completed, no influence would take place till the equilibrium was destroyed by some means, and the means is the acid which liberates the galvanic fluid from the zinc plates, and supplies it to the copper; the acid, at the same time, conducts it from the copper to the zinc, and is the medium by which it passes.

But, according to this hypothesis, if water were used in place of acid, the galvanic influence should still take place, if the equilibrium is destroyed by the water being put into the trough of a different capacity for the galvanic fluid, as it imparts it quickly to the zinc and slowly to the copper. The water not acting chemically, or very slightly so, upon the plates, no vibration, or only a very slight one, is produced; and the galvanic fluid is not propelled to the different ends of the trough with the same celerity as when the acid is used. If, however, the equilibrium was allowed to obtain among the water and metals, and no chemical action to take place, then no galvanic effect would be produced, because there would be complete rest: but the contact of the metals themselves has a chemical effect, and so would pure water have upon them, but only in a slighter degree than acid, and a current of galvanic fluid would still flow along the connecting wire, but only in a less sensible stream.

According to this theory any other substance beside metals might be employed in the trough, if the one was a better conductor of electricity than the other, and arranged in a certain order.

SECTION II.

In chemical science it has been the study of the chemist to ascertain and point out the different affinities that bodies have to each other; to show which are the strongest; what are the laws that regulate the composition and decomposition of bodies, and to discover the cause of affinity and repulsion when bodies are brought into certain positions with respect to each other.

Sir Humphry Davy was among the first who gave anything like a probable reason why the affinity of bodies might be owing to the influence of the electric fluid. That hypothesis since his time has received a considerable degree of corroboration by several authors: but though all these authors have found from experiment that electricity is intimately connected with every change of chemical action, yet they have not attempted to account for the *production* of the action, or mode of operation by the presence of the electrical fluid. They find that an action takes place when two or more bodies are brought together, but they have not attempted to show what it is in these bodies which gives rise to the action: the mode of operation is not investigated; they only find that there is weaker or stronger affinities, attractions, or repulsions, and to these they ascribe the effects which take place.

These philosophers have discovered that when a certain arrangement of metals and liquids are placed together, as in the galvanic trough, that certain decompositions and recompositions are the result, by means of a stream of electricity; but they have not shown *why* that stream should produce such effects, or why it should take place by the metals and liquids being so arranged.

My object in this investigation is to discover what it is in the bodies that is the cause of the changes which is effected by that arrangement. It must proceed from some quality or property in

the bodies employed in connection with their arrangement, which enables them to produce a current or stream of electricity. It is found that different kinds of metals will produce this effect; and that different kinds of liquids may also be employed with effect. But the metals must be arranged all in a particular order, and that a medium of communication must be established between the plates by a liquid or other conducting body; and it is found, that were but only one of the plates reversed, the stream of electricity would no longer circulate. *How* has this apparatus produced this current of electricity is the question to be answered? That it has produced it is unquestionable. Has its motion in passing through any of the acid solutions been ascertained? Is it propagated by vibrations, undulations, or by a right forward motion in the fluids? That it is propagated by undulations seems highly probable, because the fluid is stationary and cannot get forward; and as the whole surface of the plate must act on the acid fluid at the same time, the electric fluid will be communicated to every touching particle, and transmitted from it to the one adjoining successively, till it reach the next plate. Whether the electric fluid is transmitted in this way, or whether it is only a system of vibrations communicated from the plates to the acid fluid in the troughs, and by it to the plates in succession, I shall not at present pretend to know. If it is by vibrations only, then it must be supposed that the air and acid is saturated with the electric fluid; that it is only put in motion by the vibrations; and that the vibrations are so quick as to separate the elements of water, or other substances, at the sharp ends of the wires.

But how does it happen that only one element is evolved at one wire? Where goes the other? Does it go to the other wire? If the separation takes place at the positive wire, why does one gas evolve there, and not both gases rise together? Has the stream of electricity a tendency to carry the one gas forward to the other or negative point of the wire, and leave the other to rise to the surface? This seems to be the case from observing the platinum wire: but whether this be the case or not may probably be ascertained by experiment. One thing seems probable, that the electric fluid fills all space in all substances, solid, liquid, and gaseous, and that its *circulation* or current is caused by a change of temperature in the substances. This change of temperature may be caused by the plate being composed of two metals which have different capacities for caloric by being soldered together; the communication being complete, the heat of the one which has the greater capacity will be flowing into the other continually till it be saturated, and a balance be established. But as there is a communication by means of the acid in the trough for the colder caloric to escape to the next adjoining warmer plate, it will go there, and enter that plate, but that plate being also soldered to another of less capacity for caloric, it will be continually discharging its caloric into that plate, and the plate again into the acid fluid, and so on through the whole trough. The two extreme plates having wires fixed to them, the

points of these wires being brought near together enables the stream to be completed, or the circuit formed, by which the electric fluid is formed to produce such extraordinary effects.

It may still be asked what makes the stream of electricity circulate, if there is no impelling force or power? The answer is, that it is owing to the elasticity of the electric fluid, which has a tendency, from its elasticity, to produce an equal state of density in every body, and throughout the universe. The intensity of the fluid is as its density, every particle being the same wherever it is placed, but by its rarity it becomes less hot, and by compression or condensation it becomes concentrated, and has its elasticity and intensity increased in proportion to the compression.

The instance of compressed air in a syringe is a complete proof of this proposition, and instead of condensation, if the air were rarified, without increasing the quantity of caloric, it would feel colder, and it is that which produces heat in thunder storms, and condensation in the lightning.

From this theory it appears that it is the *motion* of the electric fluid that must produce the *changes* or effects on the bodies with which it is in contact.

There is a difficulty of accounting for the different capacities and dispositions that bodies have when in contact to contain caloric or electricity (for they are considered the same in different circumstances), and to part with it to the adjoining body with which it is in contact.

It is a fact that different bodies exposed to the same temperature in a room, and which show the same degree of temperature by the thermometer, will change their temperature when brought into new situations in the same room. For instance were a piece of lead and a glass containing water brought into contact, with a piece of damp cloth between them, which had formerly stood at a distance from each other, the lead would part with its cold, or rather the water would part with its caloric to the lead. The lead would absorb caloric, and the water would furnish it in the first instance, but before it took the temperature of the room it would absorb from the surrounding atmosphere the quantity wanted. Whenever two bodies have different radiating powers, then if one have the power of absorbing faster than the other radiates it will contain much heat, but it will probably also part with it quickly. If it absorbs quickly it radiates quickly. A third body may still absorb quicker than either of the two former, and also part with it quicker than either. This disposition will cause a stream of caloric to flow along, so long as the bodies do not approach a state of equilibrium. But if the last is connected so as to radiate upon the first, it will supply it with more caloric than it can absorb, and thus a constant stream will be kept up similar to that by the galvanic apparatus.

The electric spark when taken from a steam boiler is a proof of the theory, that all electricity is only caloric in a quiescent condensed state: in that state called positive it is in a higher state of conden-

sation, and to supply that quantity of heat and electricity given out by the steam, if a communication is formed between the positive or strong heat and the boiler, the boiler being in an exhausted state absorbs the greater or positive heat in shocks, or as flame on points. And if the boiler is insulated as there is no other communication, or rather the communication is diminished, the absorbing power becomes stronger, owing to the same quantity of steam being drawn off, while the means of conducting the caloric to the inside of the boiler is also cut off from the ground, and therefore it is supplied from the atmosphere by points.

But all the negative electricity is only electricity in a lower state of condensation, or of a lower temperature. It is caloric of a lower temperature than positive electricity, which is caloric of a higher temperature. And it will be found that there may be two positive electricities indicated by electrometers, and that these will attract instead of repelling each other, if these electricities are taken from such objects as are very different in temperature, so that the one electricity shall be much more concentrated and intense than the other.

Suppose electricity a very subtle fluid, extending every where, filling all empty space in bodies, and acting under a strong elastic compression. Let this fluid be supposed caloric or fire, or what gives the sensation of heat when it is accumulating on our sensative organs, and the sensation of cold when it is departing from them and diminishing; but when stationary we have not any sensation of it. It may also be supposed, that in its movement along unanimated bodies, that all the effects it produces were caused by its movements. What gives rise to these movements? comes to be the enquiry.

We perceive friction to be a powerful instrument in accumulating electricity, and what is the mode of operation? How does friction cause its accumulation? Is it not like working with a pole in a quicksand? By the motion of the pole the sand is moved, and the compressure that is acting on the water makes it rush into the empty space made by the pole, and the more the pole is agitated the more the water rushes round it and takes the place of the sand, until there is an atmosphere of water round the pole, and which will retain its situation for some time, but will be again slowly absorbed by the sand which will regain its former firmness, and an equilibrium will be again established between the sand and the water some short time after the pole has been allowed to remain at rest. It is the same with all bodies and electricity; if they are rubbed the friction opens their pores and the compressed electric matter rushes into every crevice, and by its pressure from the atmosphere retains its place for some time: even the atmosphere around the body having been disturbed, the electric matter is by the friction of the body increased beyond what it can contain, and it then extends itself in an atmosphere round the body. The friction not only creates vacuities in the pores of the body rubbed, but it also carries away what is

accumulated and gives it to the surrounding atmosphere; and if the atmosphere be dry and calm, it will be retained around the body without dispersing for a considerable time, and form as it were a cloud round the rubbed body. The bodies from which this cloud of electricity is drawn would be those in the vicinity, unless it were drawn by conductors from a distance. All bodies that are much rubbed or hammered by their vibratory motion make room for the atmosphere of electricity to rush in and form around them. The electricity is not visible to the sight but when it is moving in an insulated concentrated mass, when it jumps from one conductor to another, or when passing along a wire it makes it red hot. It is only in a vibratory or moving state that it is perceived. When we are insulated and charged with it, we do not seem sensible of it; it is only on its ingress and egress when that is not performed by points, that we feel its operations. If points are used, we may not be sensible either of receiving it or discharging it, for it flows them in a smooth imperceptible stream.

We may thus account for an atmosphere of electricity; but why should that atmosphere attract straws and feathers: or a piece of paper be fixed against a wall by it. It must be by a stream of electricity being made to revolve round the paper or wax; it makes a magnet of the body, and while the property continues, the bodies continue to attach themselves. It creates a vacuum which those other bodies supply till its property is gone: and this process is the same as that in the galvanic trough till the equilibrium of temperature is destroyed, and it is this which produces all the phenomena. Those bodies are magnets, and magnets are nothing but bodies with their parts arranged like the galvanic pile, and circulating a quantity of electric matter through them; but they have the advantage of not requiring any new supply of material, and their power is constant and does not depend on any new supply of acid, or zinc, or other material. The grand conclusion is, that the world of nature is only one large galvanic apparatus, by means of which all the changes that ever have or ever will take place are accomplished.

SECTION III.

The theory of electricity is still unascertained, and it may truly be affirmed, that notwithstanding the innumerable experiments which have been made, since the time of Dr. Franklin, that he who first wrote upon the subject appears to have had clearer and more consistent ideas of the operation of that fluid than any of the later authors. This is confirmed by the statements and quotations from his writings in a late number of your periodical. That there is but one fluid that has a tendency to extend itself throughout all bodies in the world, and that it produces its effects by its excess and deficiency when there wasany thing to cause that variation; that everything had a natural capacity for holding a certain quantity of that fluid; that it might be so altered as to contain more or less, in the same way as sponge can be made to hold more or less water by the pressure being varied.

These and other observations have confirmed me in opinion that electric matter is only a very rare elastic fluid in an unactive state, which is not perceived nor operates but when in motion. It appears even questionable whether it is elastic of itself, or whether the elasticity it possesses is not derived from the caloric imparted to it. Water becomes elastic by having heat imparted to it, and why may not electricity do the same?

That electricity is influenced by caloric in producing it in great quantities, or in causing a difference in the equilibrium which bodies naturally possess when the temperature is unchanged, cannot be denied. The main question is, whether is electricity and caloric the same element in different states of existence, or are they two simple elements of nature?

It appears for several reasons that electricity is a simple fluid, and the most subtle and insinuating of any in nature, and that it is not felt or perceived but when in motion. It is when in motion only that it produces sensible effects; that it is put in motion by bringing bodies near to each other or into contact, which have different capa cities for retaining or parting with it, which causes one substance to part with a portion, and another to imbibe it. For example, suppose in a room there were lead, wax, water, oil, mercury, and other different metals, and that the room had arrived at its fixed and steady temperature, then there would be no change of electricity, the galvanic stream would be circulating so smoothly as not to be perceptible, no motion among the parts would be perceived. But if any change of temperature were induced on any one body, it would change its state of electricity accordingly, and both the temperature and electricity of all the rest would be affected, the galvanic currents would be increased, and it would be some time before the equilibrium could be restored, both with respect to the temperature and electrical state of the bodies. Were lead and wax placed at a distance from each other, each would have its proper temperature and electricity; but if they were suddenly brought into contact, the wax would impart its heat to the lead, or rather the lead would absorb the heat from the wax with avidity, and the temperature of the wax would be reduced, while that of the lead would increase until such time as an equilibrium took place, when the lead could absorb or retain no more. This would disturb the temperature of all the other substances in the room, and an exchange would be making in all of them, until each had such a state as to give out as much heat and electricity as it received.

There can be no change of temperature without a change of electricity, and no change of electricity without a change of temperature; and though these changes may not always be discernible, yet it may be inferred from the consequences, that heat has on all bodies the power of changingt heir temperature and bulk, which cannot be done without disturbing the balance or equilibrium that exists in the electrical state of the bodies.

If electricity is considered as a very subtle fluid, set in motion by

a vibratory force induced upon it by any other body whatever; it will cause a change in the electrical state of the bodies on which the motion has been induced. If friction or rubbing of two bodies produces heat, that rubbing produces electricity at the same time, or rather I should say, it sets it in motion, by destroying the equilibrium, and thereby renders it visible, by causing a superabundance on one body and a deficiency in others, it thereby increases the stream and makes it perceptible.

It is only when there are two or more bodies whose natural state has been disturbed, which puts it in our power to discover this fluid. It is not the quantity in any body that we can estimate by the electrometer or any instrument, it is only the difference of electricity between the bodies: the excess of one above the other. For instance: if two bodies are both very highly electrified, and the electrometer placed between them, if both had the same quantity the electrometer would not be moved; but diminish the one or increase the other, and then the electrometer would shew the difference; and it must be observed that the negative is always the one where there is a deficiency, or which contains the least electrical matter; and it will be found that the one which is in excess, is always the one which has always an excess of temperature, though the difference may not be easily estimated.

When bodies vibrate with a certain velocity the heat becomes sensible; when they vibrate quicker, they begin to affect the eye and produce vision though in a solid state; but when reduced to gas and inflamed, they increase the light or vision, and also send out more heat; and this by both increasing the vibratory motion and furnishing the molecules of electricity, which are set free by flame when the bodies are consumed by combustion.

When heat is communicated to water in a boiler from the furnace through the boiler plate, it is by means of the vibratory motion that is induced on the boiler plate by the intense vibratory motion in the furnace; there being least resistance in the inside of the boiler from a deficiency of electric matter, every vibration creates a motion among the particles of the water, and makes a vacuity into which the more subtle fluid of electricity rushes in, and this continues until the interior acquires such a motion as to throw off the electric matter along with the water in steam as fast as it is communicated, or to burst the boiler if an opening is not left for its exit. But if there were a stream of cold water rushing over the top of the boiler, the heat or electricity would be carried off as quickly as it was formed, and no bursting would take place: water being a good conductor of electricity.

Electricity it was before stated would be discoverable wherever there was a difference of temperature, and that in a room where every object had the same temperature, the equilibrium would be destroyed by placing the objects in new situations with respect to each other, or connecting them by better conductors, for instance by wet threads, and the electricity would be also changed. The

galvanic circle or current is changed by the conductors, and it is easy to perceive why a damp atmosphere in a room should prevent the accumulation of electricity: it forms a far better medium of accommodation among the bodies, and dispenses the electrical matter as fast as it is collected to all the different parts of the apartment. All articles in a house or in the world, are nothing but a portion of a galvanic apparatus of various capacities for the electric fluid, and for conducting it from one body to another: the currents of electricity are circulating through the whole, and preserving them in a state of equilibrium.

The bleaching process may be explained as follows from this theory: whenever any body is bent, or its parts by any means put out of their natural position, some of the parts of the body must separate if they be placed at greater distances from each other, and other parts may be forced closer together. By agitation or action a movement among the parts is created, and electricity being a most insinuating fluid rushes into the vacant spaces by its elastic pressure, and acts as a wedge separating the parts: by continued agitation the electricity is increased, and by its density the parts are so removed as to allow the colouring matter to unite with the soap or other bodies, and be removed. It is also probable that portions of the colouring matter may be carried off by the continued stream of electrical matter, which is made by boiling to pass through the subjects boiled, in the same way as it is found to carry particles of gold or other metal with it when passing through them.

Even without a knowledge of the experiments of Mr. Crosse and Mr. Weeks, by which the influence of galvanic electricity was shown in bringing into action animal existence, it would scarcely have been possible not to have suspected that both vegetable and animal growth and existence was largely indebted to that power.

Newcastle, 24th Sept., 1842.

An Account of Improvements in Electrical Batteries. By Mr. John Cuthbertson.*

In the year 1774, Mr. E. Nairne made an electrical machine far superior in acting power to any that had been made before, and a battery more judiciously constructed and larger than any former one, with which he made a number of interesting experiments. One in particular, affords an accurate measure of the power of his battery, compared with such batteries as have been made since that time. I mean the experiment of melting or dispersing a metallic wire. His battery contained 50 square feet of coated surface; and he found that it was capable of receiving a charge so high, that the discharge melted 45 inches of iron wire of $\frac{1}{150}$ part of an inch in diameter, which answers to about $\frac{7}{8}$ of an inch in length for each square

* From *Nicholson's Journal.*

foot, which was the greatest length of wire ever melted. We have no account of this experiment having been afterwards repeated on a scale of any considerable magnitude till the year 1785, when I constructed a battery for the Teylerian Society, at Haerlem, containing 135 square feet of coated surface. With this battery 180 inches of the same sort of wire was melted, which seemed to be much more than in proportion to the size of the battery, as this was about 1·3 inches for each square foot. This battery was afterwards increased to 225 square feet of coating, and with this 300 inches of the same sort of wire melted, which was also at the rate of 1·3 inches for each square foot. Some time after this, I made another battery for the same society, containing 550 square feet of coating, composed of 100 jars of $5\frac{1}{2}$ square feet each. The same sort of wire was not tried with this; but it could be calculated from other sorts of wire which it melted, that it was capable of melting 655 inches, being also at the rate of 1·3 inch for each square foot. This increase of power, which is almost double that of Mr. Nairne's, might be attributed to the acting power of the machine; for though Mr. Nairne's machine possessed the strongest acting power of any machine made at that time, yet it could not be supposed to possess the high charging property of the Haerlem machine.

Since my return to London I have made several batteries, commonly composed of fifteen jars, each containing 168 square inches of coated surface, consequently the whole battery contains seventeen square feet of coating. This battery, according to the proportion of that made by Mr. Nairne, should fuse 6·3 inches, and in proportion to the Haerlem batteries, it ought to fuse twenty-two inches; but instead of following that proportion, it is found to fuse sixty inches, which is an astonishing increase of force. For the battery is only about one-third part of that of Mr. Nairne, and fuses a much greater length of wire; and though it is only $\frac{1}{32}$ part of that at Haerlem, yet it fuses $\frac{1}{10}$ of the length of wire. It seems difficult at first sight to account for this advantage. I have before remarked that the proportional difference between the charge of the battery at Haerlem and Mr. Nairne's, might be accounted for from the high charging power of the great machine; but the result of the last-mentioned experiments overturns that notion, as it can by no means be supposed, that a single two-feet plate machine, which I have used to charge the battery of seventeen feet so high as to fuse sixty inches, can have a higher charging power than that at Haerlem; so that it must proceed from some other cause. It might be questioned whether all the batteries were alike judiciously constructed. As to Mr. Nairne's, it certainly had faults, both with respect to the coating and the mounting of the jars; but the batteries at Haerlem were as judiciously constructed as my present one which I am speaking of, and which exceeds them in such an astonishing degree in its proportional force. The only difference between my present batteries and those at Haerlem is in the glass. They were

composed of glass blown in Bohemia, and those which I make here are of white flint glass. I mention this fact, but I am not inclined to think that the cause of the difference depends on the glass, because I remember to have melted the same quantity of wire with one jar of that kind of glass when in Amsterdam, as I do at present with white flint glass; so that it only remains now to be sought for in the manner of using or charging each battery, and here we shall probably find a means of solving this paradox.

With regard to the batteries at Haerlem, they were never attempted to be charged but in dry weather, being such as was then commonly called favourable for electrical experiments. There was no convenience in the room where the machine and batteries were used for making a fire, which was therefore ill calculated for electrical experiments: the batteries previous to charging, were made as clean and dry as possible; and if they received a charge so high as to cause a spontaneous explosion, they were then looked upon to be in their most favourable state.

It was about this time that we were told by Mr. Brooke, that a coated jar would take a higher charge when dirty, than when clean; but the degree of dirtyness was so ill defined, that I must own I never could dirty a single jar so as to answer, or to come near what was said of it; and to pretend to bring all the jars in a large battery, containing upwards of two hundred, into that state of dirtyness was never attempted; neither does it appear that Mr. Brooke ever thought of dirtying his battery jars, as he only mentions trying two small bottles, whose charging property was very differently increased by his method of dirtying.

Some time afterwards, in the year, 1792, I happened casually to discover that a coated jar, when it was a little dampish in the inside above the coating (which is always the case when a jar is fresh coated), would take a higher charge than it would do after it had been coated for some time, and was quite dry in the inside; and also, if the atmosphere was in a moist state, and the jar not dryed in the inside, it would take an equally high charge. From this it appeared evident to me, that if I could, by any means, render the inside of the jars damp, it would answer the same purpose. Breathing into a jar was tried, and the success was such, that it would receive and retain nearly double the quantity of electric fluid it could retain when dry; and in trying to fuse wires with the charge of one jar in a dry state, no more than five inches could be fused, though after breathing into it, twelve inches were fused.

This method appeared at first sight to have increased the force to more than double; but notwithstanding so evident and striking an effect, I did not think of trying what would be the result of charging a battery, after the jars had been breathed into; being deterred, as I suppose, from the idea of its being so contradictory to the common method of using batteries, which was never attempted to be done but when the atmosphere was in a tolerably dry state,

and the jars previously cleaned. But in March, 1796, being engaged in a course of experiments, when the atmosphere was so very dry that a spontaneous discharge always took place before I had a sufficient force to answer my purpose, it then occurred to me to try what the effect of breathing into the jars of the battery would be. In this trial, or research, it became necessary first to ascertain the real charge which the battery was capable of receiving before a voluntary explosion took place. The battery contained seventeen square feet of coated glass, and was composed of fifteen jars: it was found in the then state of the atmosphere to be incabable of fusing a greater length of wire than eighteen inches. But after breathing into each jar through a glass tube, it took a charge which fused sixty inches, to my very great surprise and satisfaction, as I then thought I had obtained a method of making one battery perform the functions of three; because three times the quantity of wire was fused, as appears by comparing this with what had been performed by increasing the surface of batteries by former electricians. This notion seemed to be justified, by observing in Dr. Van Marum's works, that I had enlarged his batteries at three different times; his first contained 135 square feet coating, the second 225 square feet, and the third 550; and the highest charge of the first was just sufficient to fuse 180 inches of iron wire of $\frac{1}{150}$ of an inch diameter, or six inches of iron wire $\frac{1}{40}$ of an inch diameter; the highest charge of the second fused 300 inches of the first-mentioned wire, or ten inches of the last-mentioned; the highest charge of the third fused twenty-five inches. We find that these batteries increased in power in the same proportion as the coated surface was increased. I was present when the wire was fused by the two first-mentioned batteries, but at the third not; however we have no reason to doubt Dr. Van Marum's report. These experiments supported me in my first notion, that I had discovered a new method of increasing the force of a battery to three times its usual power; but being unable to account for it to my own satisfaction, I resolved to make a course of experiments in order to throw some light on the subject.

The chief experiments which have been made on the force of batteries were by Mr. Brooke,* at Norwich, in the year 1786, and by Dr. Van Marum, in 1785 and 1795. The results were very different. Some experiments which I made in Holland, and afterwards repeated here, did not seem to confirm either of the two. All that

* Though I had read Mr. Brooke's book, as I thought, with a sufficient degree of attention when it was first published, I did not till lately observe that it contained any experiments relating to this subject, till I begun to write this paper and had occasion to look into his book for some references. I believe these experiments had escaped Dr. Van Marum's notice likewise, as I never heard him speak of them when he was making others of the same kind. Though Mr. Brooke's experiments were conducted with much skill and intelligence, they are so confusedly arranged, that this had entirely escaped my notice; and I doubt not but that it had also escaped the notice of several other electricians.

had been done either by Dr. Van Marum, or myself, was done without the help of such an electrometer as could indicate the proportional quantities of electric fluid with a sufficient degree of accuracy.

Mr. Brooke was possessed of an instrument of his own invention, with which it was possible to ascertain the comparative strength, if managed with the same dexterity as Mr. Brooke himself possesses. But this instrument came so high in price, and was so very difficult in its use, that few electricians provided themselves with it; which, perhaps, is one reason why this subject has so long remained in obscurity. I have lately had the good fortune to invent an electrometer which has all the properties that such experiments require, and is very simple and easy in its use; and with this I found myself enabled to go through such experiments as were necessary, with greater accuracy than any which had been made before.

The electrometer is represented in Plate II. G H is a long square piece of wood, about eighteen inches long, and six inches broad, in which are fixed three glass supports, D E F, mounted with brass balls, *a b c*. Under the brass ball *a*, is a long brass hook; the ball *c* is made of two hemispheres, the under one being fixed to the brass mounting, and the upper turned with a groove to shut upon it, so that it can be taken off at pleasure. The ball *b* has a brass tube fixed to it, about three inches long, cemented on to the top of F, and the same ball has a hole at the top, of about one-half inch diameter, corresponding with the inside of the tube. A B is a straight brass wire, with a knife-edged centre in the middle, placed a little below the centre of gravity, and equally balanced with a hollow brass ball at each end; the centre, or axis, resting upon a proper shaped piece of brass fixed in the inside of the ball *c*; that side of the hemisphere towards *c* is cut open, to permit the end *c* A of the balance to descend till it touches the ball *a*, and the upper hemisphere c is cut open to permit the end *c* B to ascend; *i* is a weight, weighing a certain number of grains, and made in the form of a pin with a broad head; the ball B has two holes, one at the top and the the other at the bottom; the upper hole is so wide, as to let the head of the pin pass through it, but to stop at the under one, with its shank hanging freely in *b*; a number of such pins are commonly made to each electrometer of different weights; *k* is a common Henley's quadrant electrometer, and when in use it is screwed upon the top of *c*.

It is evident from the construction, that if the foot stand horizontal, and the ball B be made to touch *b*, it will remain in that position without the help of the weight *i*; if it should by any means receive a very low charge of electric fluid, the two balls *b*, B, will repel each other; B will begin to ascend, and on account of the centre of gravity being above the centre of motion, the ascension will continue till A rests upon *a*. If the balance be set again horizontal, and a pin *i*, of any small weight, be put into its place in B, it will cause B to rest upon *b*, with a pressure equal to that weight,

so that more electric fluid must be communicated than before, before the balls will separate; and as the weight in B is increased or diminished, a greater or less quantity of electric fluid will be required to effect a separation.

When this instrument is to be applied to a jar, or battery, for which purpose it was invented, one end of a wire, L, must be inserted into a hole in *b*, and the other end into a hole of any ball proceeding from the inside of a battery as M:* *k* must be screwed upon C, with the index towards A; the reason of this instrument being added, is to shew, by the index continuing to rise, that the charge in the battery is increasing, because the other part of the instrument does not act till the battery has received the required charge.

If this instrument be examined with attention, it will be found to consist of three electrometers; and answers three different purposes, namely, a Henley's electrometer, Lane's discharging electrometer, and Brooke's steelyard electrometer; the first not improved, but the two last, which were very defective when first invented, I flatter myself are here brought to perfection. As the only use of Henley's electrometer to this instrument is, as I have said before, to shew by its continuing to increase in divergency that the battery continues to receive a still stronger charge, it required no improvement; but Lane's electrometer, in its primitive state, could by no means answer the required purpose for batteries, because the ball intended to discharge the battery, was necessarily placed so near to the ball of the battery, that dust and fibrous particles were always attracted by and adhered between the two balls, so as to retard the charging, and often render a high charge impossible: whereas, in this, they are placed at four inches asunder; and when the desired height of charge is obtained, and not before, the ball of the electrometer moves of itself nearer to the ball which is connected with the outside of the battery, and causes a discharge. The defect in Brooke's steelyard electrometer were, 1st, that it could not cause a discharge, and 2ndly, the difficulty of observing the first separation of the balls caused great error. If it were not placed in an advantageous light (which the nature of the experiments could not always permit), it would not be seen, without the attention of an assistant, which is sometimes unpleasant, and cannot always be commanded. But the instrument which I have described requires no attention or assistance, for as soon as the separation takes place between B and *b*, the ball A descends, and discharges the battery of itself.

By this combination of improvements, we possess in the present instrument all that can ever be required of an electrometer; namely, by *k*, we see the progress of the charge; by the separation of B *b*,

* A chain, or wire, or any body through which a charge is to pass, must be hung to the hook at *m*, and carried from thence to the outside of the battery, as is represented by the line N.

we have the repulsive power in weight; and by the ball A, the discharge is caused, when the charge has acquired the strength proposed.

Experiments made with a View to determine in what Degree the Charging Capacity of Coated Jars is Increased by Breathing into them before the Charging.

Experiment 1.—Prepare the electrometer in the manner shewn in the plate, with the jar M annexed, which contains about 168 square inches of coating;* put into B the pin, marked 15; take two inches of watch-pendulum wire, fix to each end a pair of spring tongs, as is represented at G *m*, hook one end to *m*, and the other to the wire N, communicating with the outside of the jar; let the uncoated part of the jar be made very clean and dry; and let the prime conductor of an electrical machine, or a wire proceeding from it, touch the wire L; then if machine be put in motion, the jar and electrometer will charge, as will be seen by the rising of the index of *k*, and when charged high enough, B will be repelled by *b*, and A will descend and discharge the jar through the wire which was confined in the tongs, and the wire will be fused and run into balls.

Exp. 2.—Put into the tongs eight inches of the same sort of wire as before, hang one pair of tongs to the hook *m*, and apply the other to the wire which forms the outside communication: take out the pin in B, and put in its stead one marked 30; all the other part of the apparatus remaining as before, and the uncoated part of the jar being previously cleaned and dried; the machine being then put in motion, the jar and the electrometer will charge, as is shown by the rising of the index as before; but as soon as the jar has received a greater quantity of electric fluid than before, a spontaneous explosion will happen without affecting the balls B *b*, because the discharge will have passed along the uncoated part of the jar from the inside coating to the outside; whence it follows, that while the jar remains in that clean state, it is incapable of receiving a charge high enough to affect the balls, or even a higher charge than it had received in the first experiment. Let the uncoated part of the jar be therefore rendered in a slight degree damp; which is easily done by breathing into the inside through a glass tube; put the machine in motion, and no spontaneous explosion will happen, but the balls B *b* will repel, as in the first experiment, and the discharge will happen from A to *a*, and pass through the wire placed in the circuit; and though it was eight inches, it will be fused in the same degree as two inches in the last experiment, namely, the wire seen red hot the whole length, and then fall into balls.

Very different degrees of fusion are caused by electric discharges, which may cause great mistakes, if not well attended to. It is proper to adhere to the degree above mentioned, and particular care

* Take out the pin in B, and observe whether the ball B will remain at rest upon *b*; if not, turn the adjusting screw at C till it just remains upon A.

ought to be taken to lay the wire, intended for fusion, straight, without any bendings or angles in it. The wire used in the two last experiments was that which is commonly called watch-pendulum wire, which is flatted; and as it approaches very near to such a sharp edge as might be supposed to affect the experiment by permitting a dissipation of the electric fluid in its passage, round wires were tried, and the result was the same.

By the last experiment it appears, that breathing into the jar had increased its charging capacity nearly in the same proportion as it had done the batteries: after breathing, it received a charge sufficient to fuse four times the length of wire it did when clean; but by the weight in the electrometer, and also by the greater number of revolutions given before the discharge happened, it might be supposed that the jar had received only a double charge.

The following experiments are intended to shew the lengths of wire which are just fused by various quantities of electric fluid at the same intensity.

Exp. 3—For this purpose a second jar was placed at the wire L, the pin marked 30 was taken out, and 15 put in its place, two inches of the same sort of wire as used in the last experiment was placed in the circuit, every other part of the apparatus remaining unaltered; the machine was then put in motion, till B begun to ascend, while it was stopped, and before A could reach *a*, one of the jars was pushed from the wire L (to do which there is always sufficient time while the electrometer is in motion), the discharge was effected, and the two inches of wire was just fused.

Exp. 4.—The jar which was pushed away in the last experiment was discharged, and placed at the wire L, as before, and eight inches of the same sort of wire placed in the circuit; the outside coating of the jars either touched each other, or had a metallic communication. All the other parts of the apparatus remained as before, and the machine was put in motion till B began to ascend; the jar was not removed as in the last experiment, but suffered to discharge with the other, and the eight inches of wire was fused in the same degree as the two inches in the last experiments.

It is evident from the position of the apparatus, that the quantity of electric fluid discharged in the last experiment must be double that of the former; yet, in repeating the experiment I had different results, which made me again suspect the edges of the wire, I therefore resolved to take round wire, and of as large a diameter as could be conveniently fused.

Exp. 5, with three jars.—Iron wire of $\frac{1}{150}$ part of an inch in diameter, and six inches in length, was placed in the circuit; three jars were placed so that the balls proceeding from their insides touched the wire L, and their outside coatings touched each other. The machine was turned till B begun to ascend, the discharge was caused, and the whole length of the wire was just run into balls.

Exp. 6, with three jars, one removed.—Two inches of the same sort of wire was placed in the circuit in the same manner as the last,

and the three jars remained; the machine was turned till B begun to ascend, then one of the jars was drawn away, consequently only two discharged, and the wire just run into balls as the last.

Exp. 7, *with four jars.*—Wire of $\frac{1}{150}$ part of an inch was taken, and four jars placed in contact with the wire L, with their outside coatings in contact with each other, and eight inches of wire was placed in the circuit; the weight in the electrometer remaining as before; the machine was then put in motion till B begun to ascend, the discharge was effected, and the wire was fused and run into balls. The experiment was repeated with the same sort of wire $8\frac{1}{2}$ inches long; the discharge was just sufficient to run it into balls.

Repeated with nine inches of the same sort of wire, and the discharge caused it to be red hot the whole length.

Exp. 8, *with four jars, two removed.*—Two inches of the same sort of wire was placed in the circuit, all the jars remaining as in the last experiment, the machine put in motion till B begun to ascend, then two of the jars were drawn away; the discharge was caused, and the wire was fused and run into balls.

Repeated with the same sort of wire $2\frac{1}{2}$ inches long, the discharge caused it to be red hot the whole length.

Exp. 9, *with fourteen jars.*—Wire of $\frac{1}{100}$ part of an inch diameter was taken, eight inches long, and proceeded according to experiment 7; it was fused and run into balls.

Exp. 10, *with fourteen jars, seven removed.*—Two inches of the same sort of wire was taken, and proceeded with according to experiment 8; it was fused and run into balls.

The results of the foregoing experiments proves sufficiently, that double quantities of electric fluid, in the form of a discharge, will melt four times the length of wire of a certain diameter: and experiments 5 and 6 prove that when one-third part is added to two, three times the length of wire was fused.

These experiments give reason to apprehend some error in Dr. Van Marum's experiments, because he found his batteries to increase in power only in proportion as the coated surface was increased, viz. that double surface of coated glass only could fuse double lengths of wire of the same diameter.

The doctor might, perhaps, have been led into a mistake in the following manner: first, he may not have charged the batteries to an equal height, as he did not at that time possess an electrometer of sufficient accuracy for that purpose; and, secondly, he may not have been aware of the different degrees of fusion caused by electric discharges, but only judged of the force by the wires being converted into balls; by which great mistakes may happen. For if a wire be taken eighteen inches long, and of such a diameter that when a jar or battery is charged to such a height as just to cause it to run into balls, much shorter lengths of that same sort of wire may be subjected to the same force, and still be only converted into balls by it; even if only seven inches were taken, nothing but balls will appear; the only difference will be, that the balls will be smaller, and dispersed

to a greater distance, which might be easily overlooked. If six inches of the same sort of wire be taken, it will be converted into balls and flocculi, or brown oxide of iron; so that to be accurate in this point, the lowest degree of fusion must be had, which is known when the charge has passed by the wire being seen red-hot the whole length, and afterwards run into balls.

Having now sufficiently proved, by experiment, in what proportion different quantities of electric fluid act upon different lengths of wire, which was required to be known, in order to explain in what proportion the charging capacity of a jar or battery is increased by breathing into it, before the charging begins, I shall proceed in the next place to explain this point.

The opinion that I had at first entertained (though supported by Dr. Van Marum's experiments), that I had found out a method of increasing the charging capacity of batteries to three times their usual force, was not supported by the facts that the usual power of a clean and dry battery, containing seventeen square feet coated surface, namely, that of fusing from eighteen to twenty-two inches of iron wire of $\frac{1}{150}$ part of an inch in diameter, will be increased by breathing into the jar, so as to become capable of fusing sixty inches. If the first-mentioned effect be taken at a mean, it will be twenty, then the increased effect, gained by breathing, will be just $\frac{2}{3}$, as determined by the wire; and experiments 5 and 6 prove, that in order to produce such an increased effect, an addition of $\frac{1}{3}$ part of the coated surface must be added to the battery, which is about 816 square inches. This would amount to an addition of fifty-four square inches to each jar; or, in other words, if that quantity of coating could be added to each jar, the same effect would be produced as when breathed into. But this would require the coating to be within an inch of the top, which would render the battery unchargeable, at least, to that degree. A battery of fifteen jars constructed in the usual manner, will, therefore, by this treatment, become equivalent in power to twenty-one jars of the same kind, if clean and dry.

To explain the effect of breathing into the jars appears to be a matter of some difficulty. This experiment has been shewn to several electricians, and different opinions have been advanced, most of which seem to imply, that breathing acts as a coating to the uncoated part, which will appear in the sequel to be absurd. Mr. Nicholson's opinion (see *Philosophical Journal*, vol. ii, page 219) comes much nearer to the truth, though it does not appear to me to be sufficient to amount to the effect produced. I admit, with him, that a spontaneous explosion over the uncoated part is most commonly caused by undulation; but that this undulation is caused by the discharging of different charged zones, willbe difficult to prove, because such zones cannot exist upon clean and dry glass.

When the uncoated part of a Leyden jar is made perfectly clean and dry, and the jar set to the conductor of a machine in action, it will begin to charge, and, while charging, the coated part of the

jar, and the wire which is connected with it, become equally charged, and each endeavours to throw off that surplus of electric fluid which is forcing into them; the coating from its edge upwards, and that part of the wire which is above the coating and within the jar, will endeavour to throw it in all directions, which will cause it to be surrounded by an electric atmosphere, increasing in density as the charge increases. This atmosphere, together with that given out by the coating, fills the whole jar. Part of the electric fluid forced into the coating enters the surface of the glass, but the uncoated part, being clean and dry, both withinside and without, the inside resists the fluids entering its surface, which is kept suspended at a distance, because the natural electric fluid contained on the outside, finds no means of escape. But the action of the machine still continuing, presses it still closer to the surface, and at last overcomes that resisting force, and some of the particles on the outside give way, which causes an undulation in the inside, and the electric fluid closes instantly in upon its inside surface, and forces a greater quantity from the outside. Flashes, or coruscations, are thus caused, which are always seen when a jar is charging in the above mentioned circumstances: the charge still continuing to be made, forces another quantity from another part of the outside of the jar, and causes a second coruscation and undulation, which may be so strong as to cause a spontaneons discharge; or two or three more coruscations and undulations may happen, before the discharge, according to the steadiness or unsteadiness of the action of the machine, the quantity of electric fluid thrown off from the outside at each undulation, and also the degree of dryness and cleanness of the uncoated part of the jar. A discharge sometimes happens without having previously occasioned any perceptible coruscation. This is the case when the first undulation has been so strong, as to cause the whole discharge with the first coruscation, the one being so quickly followed by the other that it is imperceptible.

A jar will sometimes, while it is charging, give a great many small coruscations, quickly succeeding each other, which afterwards cease without having caused a spontaneous explosiou, though the action of the machine be continued. This happens when the uncoated part is nearly clean and dry, but not perfectly so; its surface still containing some conducting particles, but not so connected that the electric fluid can pass from one to the other without leaps, or small coruscations on the outside, which permit the electric fluid to spread gradually over its inside surface, and prevent the undulations from being so strong as to cause a discharge.

After this explanation of the cause of the flashes, or coruscations, which are seen upon the uncoated part of a jar while charging, and also that such coruscations produce undulations, which terminate in a spontaneous explosion ; it remains now to explain how a jar is charged when the coruscations are prevented by breathing upon the uncoated part.

When a coated jar is breathed into, and then subjected to the

process of charging, the electric fluid is forced into it along with the wire in the inside of the coating, where it instantly and equally spreads itself over the whole coated part, and at the same time, though with difficulty, and consequently gradually, it spreads itself over the uncoated part, taking the condensed film of humidity for its conductor, as it proceeds from the edges of the coating upwards towards the mouth of the jar, according to the arrangement of the particles of moisture, and rises higher or lower, depending entirely on their arrangement, and the force with which it is repelled from the machine. If the conducting particles be almost uniformly diffused over the uncoated part, the whole jar, in the inside, will become charged, though the uncoated part will be charged in a much less degree than the coated, on account of the imperfection of the conducting particles which has adhered to its surface; no coruscations will be perceived on account of the gradual and equal diffusion of the electric fluid over its inside surface: and though the charging be continued, yet, if the exhaled conducting particles be favourably diffused, no spontaneous explosion will happen from one coating to the other, along the uncoated surface, but the jar will either be perforated, or, if it be of sufficient strength to resist that effect, the electric fluid will be seen to run in a stream over the mouth of the jar, as quickly as the machine supplies it. Whenever a spontaneous electric explosion happens, it must be from a body of sufficient bulk and conducting property to contain that quantity of electric fluid at that point from which it explodes, otherwise no explosion ever happens. But the humid conducting particles are just sufficient merely to admit the electric fluid, by the action of the machine, to be spread over the surface of the glass, but in no part of sufficient density either to receive or contain an explosion. If, therefore, a spontaneous explosion do happen, it must either proceed from the inside coating, or the wire which is connected with it to the outside; and, if we examine the state of the coating, we shall understand, that the edge of the coating (from which part only it is ever possible to explode), and also above it, to a short distance upwards, is as strongly charged as the coated part; and by the action of the machine it is so strongly loaded with electric fluid, that it is repulsive in all directions, which keeps back, or entirely stops, a spontaneous explosion from the edge of the coating. With regard to the wire, the only place from which it explodes spontaneously, is that part which is nearly of an equal height with the edge of the mouth of the jar. The fluid is nearly as much condensed on this part as on the other, so that an explosion from the wire is hindered by the same cause as from the coating. A jar, under such circumstances cannot, therefore, explode spontaneously; but the fluid will run over the edge of the jar as quickly as the machine furnishes it, when its charging capacity is full.

I have stated, at page 530, that a jar of the dimensions there given, being clean and dry, can only contain a charge sufficient to

fuse two inches of a certain wire, and when breathed into, its charging capacity will be so much increased, that it will contain a charge sufficient to fuse eight inches of the same sort of wire; and a battery of fifteen jars, in the first-mentioned state, can only fuse twenty inches, and in the last-mentioned, sixty inches. This increased charging capacity proceeds, no doubt, from the particles of moisture, though not from their acting as a coating, as has been supposed, but by their being brought into a state or capacity of resisting a spontaneous explosion, so that a stronger charge is forced in upon the coated part. Some of the electric fluid which was forced upon the uncoated part to a certain height (perhaps half an inch, more or less, according to the degree of dampness, and the saturation of the particles) may, indeed, be discharged along with that from the coated part; but this is of little importance, and by no means capable of producing that increased effect, which, as I have shewn by experiment, would require an addition of seven jars to a battery of fifteen.

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*Electro-Magnetic Locomotive Carriage.**

A TRIAL of this very ingenious machine, constructed by Mr. Davidson, was made on Thursday week, on the Edinburgh and Glasgow Railway, in presence of a number of gentlemen, many of whom were eminent for their scientific knowledge. The construction of the carriage is the first attempt which has been made in this country to apply the powers of electro-magnetism to railway traffic, and from the success which attended this trial sanguine hopes may be entertained that the period is not distant when it will either supersede, in many cases, the employment of steam, or lend a powerful aid to this mighty instrument in all the operations in which it is at present employed. The carriage was impelled along the railway about a mile and a half, travelling at the rate of four miles an hour, a rate which might be increased by giving greater power to the batteries, and enlarging the diameter of the wheels. We understand that the carriage was built at the expense of the Railway Company, and we cannot but congratulate them in having the discernment to employ Mr. Davidson, a gentleman of much practical knowledge and talent, to whose genius great discoveries have been made in electro-magnetism, by whom the carriage was projected, and to whose unwearied exertions the practicability of the scheme is almost placed beyond a doubt.

The dimensions of the carriage is sixteen feet long by seven feet wide, and is propelled by eight powerful electro-magnets. The carriage is supported by four wheels of three feet in diameter. On each of the two axles there is a wooden cylinder, on which are fastened three bars of iron at equal distances from each other, and extending

* *Edinburgh Evening Courant.*

from end to end of the cylinder. On each side of the cylinder, and resting on the carriage, there are two powerful electro-magnets. When the first bar on the cylinder has passed the faces of two of these magnets, the current of galvanism is then let on to the other two magnets: they immediately pull the second bar until it comes opposite them. The current is then cut off from these two magnets, and is let on to the other two. Again they pull the third bar until it comes opposite, and so on—the current of galvanism being always cut off from the one pair of magnets when it is let on to the other.

The manner in which the current is cut off and let on is simply thus:—At each end of the axles there is a small wooden cylinder, one-half of which is covered by a hoop of copper; the other is divided alternately with copper and wood (three parts of wood and three of copper). One end of the coil of wire which surrounds the four electro-magnets, presses on one of the cylinders, on the part which is divided with copper and wood; the other end of the coil presses on the other cylinder in the same manner. One end of the wires or conductors which comes from the battery, presses constantly on the undivided part of the copper on each cylinder. When one of the iron bars on the wooden cylinder has passed the faces of two magnets, the current of galvanism is let on to the other two magnets, by one end of the coil which surrounds the magnets, passing from the wood to the copper, and thereby forming a connexion with the battery. This wire continues to press on the copper until the iron bar has come opposite the faces of the two magnets, which were thus charged with galvanism. On its coming into that position, the current is cut off from these two magnets, by the wire or rod of copper passing from the copper to the wood, and thereby breaking the connexion with the battery. But when the wire or rod of copper leaves the copper on the one cylinder, it leaves the wood, and passes to the copper on the other cylinder at the other end of the axle, and in so doing connects the other two magnets with the battery, and they pull the next iron bar in the same manner. At the other end of the carriage there are other four magnets, and wooden cylinder, with iron bars arranged in the same manner.

The battery which is used for propelling the machine is composed of iron and zinc plates immersed in dilute sulphuric acid, the iron plates being fluted so as to expose greater surface in the same space. The weight propelled was about six tons.

Method of detecting the Adulteration of Cane or Beet-root Sugar with Sugar of Fecula. By M. E. Kranty.*

[To detect the Admixture of these two kinds of Sugar.]

To detect the presence of the sugar of fecula, two grammes of the suspected specimen are to be dissolved in a flask, in thirty grammes of distilled water; the liquor is then to be filtered, and two deci-

* *Journal de Chimie Médicale*, 1842.

grammes of potash in alcohol and one decigramme of sulphate of copper are to be added; the whole to be well stirred about to accomplish complete dissolution. The flask is afterwards to be corked, and the subsequent appearances observed.

Should the sugar under examination contain sugar of fecula, a red precipitate will be observed some time after mixing; and if we operate on a large quantity, the transformation of the cupreous salt into a protoxide will be completely accomplished in twenty-four hours. The solution loses all its blue or green colour, and not a trace of copper is to be found in it.

If we operate on pure sugar from the cane, or from beet-root, no red precipitate is formed from the same process, even after eight days' standing. If the mixture contain equal parts of the pure and impure sugars, the precipitation will be completed in about twenty hours. If the impure be only 2·5 per cent., a slight red precipitate is observed in twenty-four hours; but the solution will not become colourless in less than eight or ten days.

Thunder Storm at Bristol.

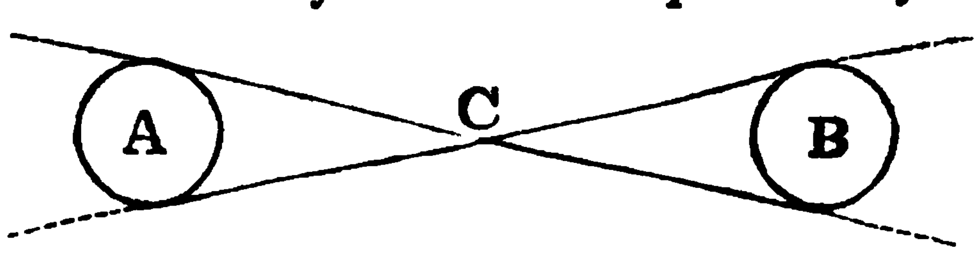
During the severe thunder storm on Thursday fortnight, a serious accident occurred at the Great Western Cotton Works, St. Philip's, Bristol. At about twenty minutes before five o'clock in the afternoon, the electric fluid struck the meter-house, and completely destroyed the large gas meter, the erection of which had just cost the company £130. The iron face of the meter was split in pieces, and the gas which was in the cylinder (fortunately it was turned off at the main) being ignited by the electric fluid, exploded, and large pieces of iron, one of them weighing about 100lbs., were thrown with such violence against a wall twenty-five feet distant as to produce considerable indentations in portions of the masonry. The meter-house is erected in front of the weaving room, in which between 500 and 600 girls were at work at the time. The main gas pipe, which is attached to the meter, passes through the wall and descends beneath the floor of this room, and as a large portion of it was blown off, it seems almost a miracle that no lives were lost. Several of the girls fell fainting on the floor, others rushed to the door to escape from the factory, and all was confusion and alarm, but no one was hurt.

THE ANNALS
OF
ELECTRICITY, MAGNETISM,
AND
CHEMISTRY;
AND
GUARDIAN OF EXPERIMENTAL SCIENCE.

NOVEMBER, 1842.

On Gravitation.

SUPPOSE that an elastic compressing fluid pervaded the universe, such as electricity, or some modification of it, the following communication is an attempt to demonstrate that were a soft *inelastic* body, semi-fluid, introduced into it, that the inelastic body would be equally pressed on all sides, and would take the form of a globe; and secondly, that were two or more bodies, as A and B, placed in that elastic atmosphere, such as the earth and sun, that the pressure of the elastic atmosphere on each body would not be equal in every direction as on the single body; that the pressure in a direct line between the bodies A and B,



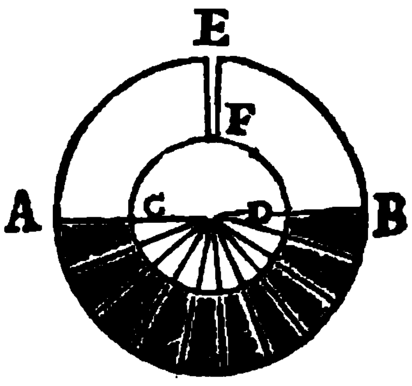
tending to keep them asunder from each other, or from the centre C, would not be equal to the pressure in the opposite direction, forcing them towards that centre.

This is the conclusion arrived at, which is in opposition to the generally received opinion, that action and reaction in all cases is equal and contrary in fluids as well as in solids. It is generally conceived that the fluid exerts the same force to press A and B asunder as it does in pressing them together, which I have attempted to show is not the fact; and my object in this communication is to give the reasons I have for this opinion to the public, so that they may be corrected if wrong, and the fallacy of the reasoning pointed out, or its truth confirmed.

That a single soft *inelastic* body, if it existed alone in an elastic compressing atmosphere, would be forced into the form of a globe by the elastic fluid pressing it equally on all sides, is a proposition that will not be disputed: but that if *two* bodies were introduced into that atmosphere, then the pressure on them would not be equal

on all sides, is a proposition that is not so obvious, but it is what is here attempted to be demonstrated; and not only that the forces are different, but that these bodies are forced together with a force inversely as the square of their distances, and directly as the masses; thereby according with all the laws which are attributed to gravitation.

Let A B represent a globe with a hollow vacant space within it, then there would be a compressing force from the atmosphere of about sixteen pounds upon every square inch of surface, tending to compress it to the centre, which is supposed to be a vacuum. But this compressing force would continue to press on the exterior surface of this globe, however much diminished the interior vacuum was made, or till there was no vacuum. Thus the pressure upon the solid globe is seen to be the same as upon one which is exhausted, of whatever size the exterior be, while the exterior is unaltered.



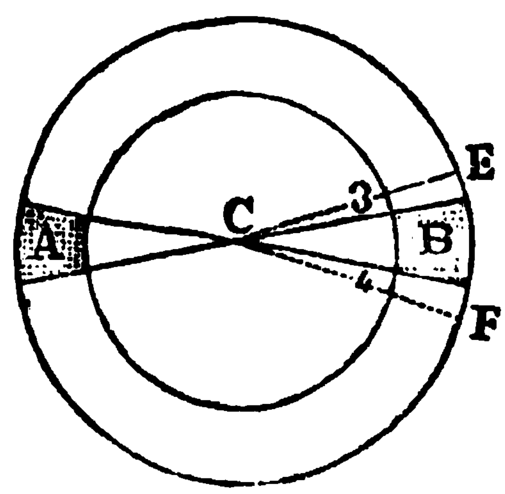
All this will readily be granted: but suppose now that a small hole E F, was made from the exterior to the interior space, and a communication made from the atmosphere to the vacuum within the globe, the elastic atmosphere would rush in immediately, and exert a force by its elasticity on the interior hollow surface equal to sixteen pounds upon the square inch, the same as on the outside surface. This, I conclude, will also be granted as a truth: but it is evident that, as the outside and inside surfaces are to each other as the squares of the outside and inside diameters, therefore the pressure inward to the pressure outward would be as the outside surface to the inside surface, and in all cases will be proportional to the squares of the diameters. Thus, were the internal diameter two feet, and the outside diameter three feet, then the pressure inward to the pressure outward would be as the square of two to the square of three, or as nine to four.

From this it follows as a corollary, that any hollow globe of soft compressible matter would be pressed together, if the consistency of the materials were not such as to withstand it; and that all hollow inelastic vessels have a greater force pressing them inwards than outwards, when they possess any considerable thickness of material. It is not true, therefore, that the parts of a hollow globe are pressed with the same amount of force from the centre as to the centre; but in a ratio as the squares of the inside to the outside diameter, or as the surfaces to each other.

Let the hollow globe now be supposed to be formed of frustums of pyramids, all pointing to the centre, as shown in the figure. The pressure inward on each of the frustums, to the pressure outwards, would still be as the outside surface of the base of each to the surface of its small end; and these are still as the square of the outside diameter to the square of the inside diameter.

This conclusion, it is supposed, will also be readily granted, there being no alteration in the figure, and the base of each pyramid being to its top as the whole outside to the whole inside surface.

Suppose farther, that one of the frustrums were abstracted; this only widens the communication between the external atmosphere and internal space, still the same proposition would hold with regard to the external and internal forces operating on the remaining frustums. In the same way let all the remaining frustums but the two opposite ones be abstracted from the globe, as in this figure, still the same ratio will hold with these; the pressure inward to the centre from the outside, will be to the pressure from the centre outwards, as the outside surface to the inside surface of the frustums, or as the square of the outside diameter to the square of the inside diameter.



Were these two bodies globes, or any other form, there is no reason why the same law should not hold good with regard to them.

It is evident that as the inside diameter or surface approaches the outside, that the shell of the globe gets thinner; and when the surfaces coincide, then the inside force or pressure is just equal to the outside pressure, and then they will balance each other, but not till then.

It is also evident that the greater the distance between the bodies A and B, or the farther they are from the centre, C, that the less will be the difference between the exterior and interior surfaces, and that this will be inversely as the square of the distance between the bodies, or which is the same, from the centre C.

It is also evident, that the larger the mass when the distance is the same, the greater must be the exterior pressure over the interior; because the greater is the difference between ends of the frustums of the pyramids, or the greater is the angle which each body subtends at the other, or at the centre C.

We have thus arrived at the same conclusion with regard to the laws of forces by which matter is governed, as the Newtonian theory of attraction or gravitation requires; namely, that the power of the forces by which bodies are made to approach each other, are inversely as the squares of their distances, and directly as their mass, or quantities of matter; and this, by only supposing the existence of an elastic compressing medium extending throughout space, which is now admitted by many of the philosophic world to exist.

This pressure to a centre between bodies must necessarily exist if this is a correct demonstration: it flows from the nature of a compressing elastic fluid: and hence there would be no necessity for attraction, for both cannot have place. If the forces, which I have attempted to show, have actually a place, then that of attraction can

have no place, as the former is capable of accounting for all the phenomena upon a much more intelligible and rational principle than the latter.

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*Theory of Saponification.**

AWARE of the importance of investigating the theories involved in the chemical arts, with a view to their ultimate improvement on scientific principles, we had formed the design of embodying the later observations of chemists on the oily substances, when an essay of Professor Liebig's appeared in the "Annalen der Chemie und Pharmacie," for March, 1841, which contains an excellent view of the theory of saponification; we therefore translate the essay nearly entire. J. C. B. and M. H. B.

What was known of the nature of saponification previous to the commencement of the present century, amounted to nothing, excepting the important discovery, by Scheele, of the *sugar of fats*, now called the *hydrated oxide of glyceryl* (glycerule). Chevreul began in 1813, a series of investigations on soaps, which have not only thrown a clear light on this portion of chemistry, but have also led the way to the most brilliant discoveries in the whole province of organic chemistry. We are indebted to him for the present predominating principle in all organic researches, viz.: to subject a body to a series of changes, and to ground its composition on the ascertained connexion of these changes.

Chevreul proved that all fats comprehended under the terms grease, oil, and tallow, consist of three materials united in the most varied proportions, one of which, *oleine*, at common temperatures, and below 32° Fahr., is always fluid, the others solid, the one called *stearine*, the other *margarine;* distinguished from each other by their melting points, and the different acids they give rise to by decomposition. These fatty substances are each composed of a peculiar fat acid, united to a compound oxide, the oxide of glyceryl, and being salts, are subject to decomposition like ordinary salts.

Decomposition ensues when a fat, *i.e.*, a compound of oxide of glyceryl, is treated with an alkali, or with oxide of lead or zinc; the alcalies, or metallic oxide, combining with the fat acid; the former constituting soluble salts, or soap, the latter insoluble salts, or plasters. The oxide of glyceryl, at the moment of its separation from the fat acids, takes up water and forms hydrated oxide of glyceryl.

The weight of the hydrate of glyceryl, added to that of the hydrated fat acids, amounts to more than the weight of the fat employed; the increased weight arising from the water entering into combination with the glyceryl and fat acids.

In the saponification of fats by alcalies, no other products are formed, and the operation is conducted equally well in vacuo, or in

* "Journal of the Franklin Institute."

the air. With strong alcaline lyes, the soap separates from the concentrated fluid, and collects on its surface, while the glyceryl remains dissolved in the alcaline solution. The soap remains dissolved in a weak and hot alcaline lye, but on cooling, the whole congeals to a gelatinous, translucent mass.

Soaps are *solid and hard*, or *soft*. The last are obtained from drying oils, and contain potassa as a base, and to give them more consistence, tallow and fat oils are added, which form solid soaps. The hard soaps contain soda, and are prepared with fat oils, tallow, &c.

Soda soaps are made in England and France directly by soda and fats, in Germany by decomposing potash soaps with chloride of sodium. Commercial soaps from vegetable fats consist of oleated and margarated alcalies; those from animal fats are salts of stearic, margaric and oleic acids, with an alcaline base.

Potassa and soda soaps are readily soluble in hot water and alcohol; the addition of a quantity of water to the aqueous solution produces a precipitation, the neutral salts of stearic and margaric acid, decomposing into free alcali, which remains in solution, and acid steorate and margarate of alcali, which precipitates in form of pearly, crystalline scales.

Potassa soaps are more soluble in water than those containing soda. Stearate of soda may be regarded as the type of hard soaps, and when in contact with ten times as much water it suffers no striking change. Stearate of potassa forms a thick paste with the same quantity of water. Oleate of soda is soluble in ten parts of water; the oleate of potassa dissolves in four parts, and forms a jelly with two parts, and possesses such a strong affinity for water, that 100 parts absorb 162 in a moist atmosphere. Margaric acid acts similarly to stearic. It follows from this that soaps are soft in proportion to the oleates, and hard in proportion to the stearates and margarates they contain. Soda soap exhibits a peculiar behaviour to a solution of common salt; it loses the power of being penetrated by, or dissolving in, a solution of salt of a certain strength, and this remarkable action is an important condition in its manufacture, on which depends the separation of all free alcali and oxyde of glyceryl, its content of water, and the state in which it is brought into the market.

If a piece of common hard soap, cut into pieces, be put into a saturated solution of salt, at ordinary temperatures, it swims upon the surface without being moistened, and if heated to boiling, it separates without foam into gelatinous flocculæ, which collect on the surface, and upon cooling unite into a solid mass, from which the solution flows off, like water from fat. If the flocculæ be taken out of the hot fluid they congeal on cooling into an opaque mass, which may be pressed between the fingers into fine laminæ without adhering to them. If the solution be not quite saturated, the soap then takes up a certain quantity of water, and the flocculæ separate through the fluid in boiling. But even when the water contains $\frac{1}{400}$ of common salt, boiling produces no solution.

If the soap be boiled in a dilute and alcaline solution of salt, and suffered to cool, it again collects on the fluid in a more or less solid state, depending on the greater or less concentration of the solution, *i.e.*, on the quantity of water taken up by the soap. By boiling the dilute salt solution with soap for a considerable time, the watery flocculæ swell up, and the mixture assumes a foaming appearance; but still they are not dissolved, for the solution separates from them. The flocculæ, however, have become soft and pasty, even after cooling, and their clamminess depends more or less upon the quantity of water they have taken up. By still continued boiling this character again changes, and in proportion as the water evaporating renders the solution more concentrated, the latter again extracts the water from the flocculæ; the liquid continues to foam, but the bubbles are larger. At length a point is attained when the solution becomes saturated; before this, large, iridescent bubbles are observed to form, and in a short time all foam disappears; the liquid continues boiling without foam, all the soap collects in a translucent mass on the surface, and now the solution and soap cease to attract water from each other. If the plastic soap be now removed and cooled, while the solution is pressed out, it has become so solid as scarcely to receive an impression from the fingers. In this state it is called *grain-soap* (Kernseife).

The addition of salt, or its solution, or a concentrated alcaline solution of soap in water, precipitates the soap in gelatinous flocculæ, and the mixture behaves precisely like solid soap boiled with a dilute solution of salt. Carbonated and caustic potassa act exactly like salt, by separating soap from the alcaline fluid, in which it is absolutely insoluble.

The application of the above to the manufacture of soap is evident. The fat is kept boiling by an alcaline lye until all pasty matter disappears, but the lye should only have a certain strength, so that the soap may be perfectly dissolved in it. Thus tallow may be boiled for days in a caustic potass-lye, of sp. gr. 1·25, without saponifying; if the lye be stronger, a partial saponification takes place, but being insoluble in the fluid, it floats upon the surface in a solid mass; by the gradual addition of water and continued boiling, at a certain point the mass suddenly becomes thick and clammy, and with more water a kind of emulsion is formed (Seifenleim), which continued heating renders perfectly clear and transparent, if a sufficient quantity of alcali be present. In this state it may be drawn into long threads, which, on cooling, either remain transparent, or are more milky and gelatinous. As long as the hot mass suffered to drop from a spatula exhibits a cloudiness or opalescence, the boiling is continued or more alcali added. When excess of alcali is present, the cloudiness arises from imperfect saponification or want of water; the former is shown by dissolving a little in pure water, which becomes perfectly clear when the whole is saponified; if the lye contain lime, the mixture is also clouded, but the addition of carborated alcali instantly clarifies it.

In order to separate the soap from water, free alcali, and oxide of glyceryl, a large quantity of salt is generally added to the boiling mass, on each addition waiting until it is dissolved; the first addition increases the consistency of the mass, while each successive portion renders it more fluid, till it looses its threading character, and drops from the spatula in short thick lumps. As soon as the congelation is complete, *i.e.*, when gelatinous flocculæ separate from a clear watery liquid, the fire is extinguished, the soap suffered to collect on the surface, and cooled either on the liquid, or ladled out, and suffered to get solid,

In the former case it is impure from water, free alcali, and other impurities of the lye, and is therefore ill adapted for commerce, although sufficiently good for domestic purposes. As in other chemical operations a precipitate is purified by boiling it in a fluid on which it is insoluble, so soap is purified by a solution of salt rendered alcaline.

The soap of the first boiling is either re-dissolved in weak alcaline lye, and precipitated by salt several times, or it is boiled with an alcaline solution of salt several times, by which means it is rendered much purer. When the saponified fluid is made with potassa, the salt (chloride of sodium) operates in a two-fold manner: it dissolves in the pasty liquid, and decomposes with the potassa salts of the fat acids, forming on the one side cloride of potassium, and on the other soda, or soda-soap. That a decomposition takes place is evident from the altered consistency of the fluid mass. Since chloride of potassium has not the property of separating soap soda, a larger quantity of salt is added. When potassa-lye is employed in soap-making, the first salting requires more than twice the quantity of salt.

In the preparation of potash soaps, a concentrated potassa lye is employed for separating the soap. Acetate, or tartrate of potassa, may be employed on a small scale. In the manufacture of soaps, the saponification of the fats is not completed by the first treatment with weak lyes; and the subsequent repetition of fresh lyes, beside purifying, also renders saponification more perfect.

In the saponifying olive and other oils, the mixture often attaches itself to the bottom of the vessel, and is burned; in these cases the alcaline lye is previously mingled with salt, so that the forming soap is obtained in a state of fine division, and yet prevented from forming a perfect solution. For common house use, soap of the first boil is only treated once with salt; that for commercial purposes is suffered to swell up in a weak salt lye, by means of which it takes up fifteen to twenty per cent. water. Grain soap (Kernseife of the Germans) is generally coloured bluish, or greenish, from sulphuret of iron, or copper, or from iron, or copper-soaps. By cooling, these colouring matters collect more or less in certain points, which gives a marbled appearance to the hard soap. Marbling is generally produced by the addition of sulphate of iron, or peroxide of iron, to the still soft mass.

For white soap, it is rendered fluid by heating it in a saline, alca-

line lye, and kept in the covered vessel until all the colouring matters have subsided. The more water the soap has taken up in this operation, the more perfect the separation of the impurities, the whiter the soap. Now, since this water is not separated, but sold in the soap, it follows that it has much less real value than the grain soap. The white soap contains from forty-five to seventy, marbled soap from twenty-five to thirty-five per cent. water.

The manufacture of soft soap is the simplest of all. The drying oils, either alone, or mixed with train oil, tallow and other fats, are kept boiling with dilute potassa lye until the saponification is completed, *i.e.*, a mass is formed which draws into long transparent threads. Particular care is had in its preparation to the dilution of the lye, for all soft soaps are insoluble in moderately strong potassa lye, and may be precipitated from their solution by the addition of strong lyes. The fluid would therefore appear cloudy, milky, with an excess of strong lye, and by adding water would become pasty, or gelatinous. A deficiency of alcali produces an acid oleate of potassa, which attaches itself in thick masses to the bottom of the vessel; but an addition of lye changes it into a neutral salt. Oxide of glyceryl is not separated from soap, although it might be done by the final use of strong alcaline lyes.

The soft soaps of commerce have a greenish, or greenish brown colour; they are transparent in thin laminæ, shining, soft but not fatty to the touch, of a peculiar odour, and have an alcaline re-action. Tallow is often added to them, which disseminates crystalline particles of stearate of potassa; communicating to them a peculiar grained character. Chevreul and Thenard found in commercial soft soap 39·2 to 44 per cent. oleic and margaric acids, 8·8 to 9·5 potassa, and 46·5 to 52 water. They always contain hydrated oxide of glyceryl and delphinate of potassa, derived from train oil, whence their peculiar odour.

When an alcaline soap is mixed with an earthy, or metallic salt, volumnious white or coloured precipitates ensue, in which the alcali is replaced by the earth, or metallic oxide. Thus the salts of lime, magnesia, &c., throw down lime, magnesia, &c., soaps. Hence the curdling appearance, when soap is used with hard waters, arises from the union of the lime or magnesia they contain with the fat acids. If carbonate of lime be in the water, it may be thrown down by a little caustic potassa or lime, which will render it softer; if sulphate of lime, or a magnesia salt be present, pearlash, (ash-lye) will separate the earths.

Description of a new Universal Photometer. By Dr. Charles Schafhaeutl, *of Munich Assoc. Inst. C. E.**

The inadequacy of the photometric instruments invented by Pictet, Rumford, and others, is universally acknowledged. The bromide

* Trans. of the Inst. of Civ. Eng.

of silver, as used by Sir John Herschel, although extremely sensitive, is only slightly affected by artificial light.

These circumstances induced the author to complete the present instrument, which he contemplated about twelve years since.

The intensity of the undulations of gaseous fluids, as well as that of the air, is proportional to the amplitude of the oscillations, or, more properly, to the square of the amplitude.

A wave of light striking the retina must create a similar vibratory motion in the nerves of the retina, because the velocity of the molecular movement of the nerves depends upon the force with which they have been struck by the original wave, and if this velocity could be measured, it would show at the same time the intensity of light.

It is scarcely possible to obtain a direct accurate measurement of this velocity, but if the time during which the vibratory motion of the nerves ceases be ascertained, the velocity of the vibrating molecules, and, therefore, the intensity of light may be determined; because the duration of an impression on the retina is dependent on the resistance which the molecules of the nerves oppose to every force striking them; but as this resistance of the nerves increases as the square of the velocity, four times the momentum, or intensity, is necessary to double the time of duration; or, in other words, the intensity of the pencil of rays is as the square of the time of the duration of that impression made on the nerves of the retina.

The new photometer consists of a brass bar fixed vertically in a stand, carrying at its upper end a small tube in two parts, which may be lengthened from five to ten inches, if requisite. This eye tube has at each end a sliding plate pierced with holes of corresponding diameters. From the bottom of the bar a projecting arm sustains the lower end of a strip of rolled steel, eighteen inches long, $\frac{5}{16}$ inch broad, and $\frac{1}{32}$ inch thick; this has at the upper end a thin plate, pierced with a small hole, corresponding with the holes in the sliders, and standing one-eighth of an inch from one of them: upon the main bar is a prism with a slit in it, through which the strip of steel passes; this prism can be moved up and down, by a rack and pinion, so as to lengthen or shorten the vibrations of the strip.

The method of using the instrument, is to adjust the two holes at the opposite ends of the horizontal eye tube, so that they perfectly correspond, and do not permit any rays of light to enter, unless the plate at the extremity of the spring be pushed aside. The light to be compared is then placed at a certain given distance behind the plate, so that by bringing the axis of the hole which is pierced in it into the axis of the tube, a small pencil of light may enter the pupil of the eye. The prism is then placed at 100 of the scale on the side of the brass bar, and the steel strip caused to vibrate gently. A luminous disc immediately appears, accompanied by scintillations, which are caused by the impressions on the retina being interrupted by dark intervals: the prism is then gradually raised, until the

length of the vibrations of the strip being diminished, and the velocity increased, the luminous disc appears perfectly steady and clear. The length of the vibrating portion of the strip is then read off by the verniers marked on the brass rod, and compared with the whole length of the spring, measured from 100, which is considered as unity. The number of the vibrations to be computed from the found length of the spring, are inversely to the numbers of vibrations of the whole length, as the squares of their relative lengths. Hence are constructed the formulæ for calculation, which are given at length in the communication.

A fresh luminous impression is made on the retina as often as the circular aperture in the screen on the top of the spring cuts the axis of the tube. If the duration of the small vibration of the nerves of the retina is shorter than the time of a vibration of the spring, a dark interval appears between the two luminous impressions. In this case the vibration of the spring is shortened until the next impression returns just as the first ceases, and therefore the dark interval disappears; then by measuring the length of the shortened spring, the number of vibrations can be computed, and from them the intensity of the light.

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*Crane's Anthracite Iron.**

Decision of the Court of Common Pleas, affirming the validity of the Patent granted to George Crane, Esq., for Smelting Iron with Anthracite Coal. Delivered at Westminster Hall, June 13th, 1842, by Lord Chief Justice Tindall.

The Lord Chief Justice:—This was an action on the case for the infringement of a patent granted to the plaintiff on the 28th of September, 1836, for an improvement in the manufacture of iron. The declaration was in the usual form, and the defendants pleaded thereto, first, that they were not guilty; secondly, that the plaintiff was not the first and true inventor of the said improvement—upon each of which pleas issue was joined; thirdly, after setting out at length the plaintiff's specification, the defendants pleaded that the alleged improvement therein described was not a new manufacture, invented by the plaintiff within the intent and meaning of the statute, as to the public use and exercise thereof in England—which allegation was traversed by the plaintiff in his replication; fourthly, the defendant pleaded that the nature of the plaintiff's invention, and the manner in which it was to be performed, were not particularly described, or ascertained by the plaintiff, in his specification—upon which plea issue was joined, and in their last plea, the defendants, after referring to the plaintiff's specification before set out in the third plea, stated the grant of letters patent, dated September 11,

* From the Cambrian.

1828, to one James Beaumont Neilson, for an improved application of air to produce heat in fires, forges, and furnaces. where bellows and other blowing apparatus were required—that Neilson's invention was the production and application of a hot-air blast, and was in public use with Neilson's license in the smelting and manufacturing of iron from iron stone, and was the hot-air blast in the plaintiff's specification mentioned—that the plaintiff could not use the hot-air blast mentioned in his specification without Neilson's license, and that he had obtained such license before the grant of his letters patent, and that the using by the plaintiff of the hot-air blast, in the smelting of iron from iron stone, combined with anthracite or stone coal, as mentioned in his specification, was a using and and imitating of Neilson's invention, whereby the plaintiff's invention is void. The plaintiff replied to this last plea, that Neilson's invention was not the same hot-air blast, and that the machinery and apparatus adapted for the application thereof, mentioned and referred to in the plaintiff's specification, was not, nor was the using by the plaintiff of the invention, as described in his specification, a using and imitating of. Neilson's invention, described in Neilson's specification; which allegation is traversed by the defendants in their rejoinder. At the trial before me, a verdict was entered for the plaintiff on all the issues, subject to the opinion of the Court, upon the evidence given at the trial, as contained in a report agreed upon between the parties—the Court being at liberty to draw the same inference from it as a jury might draw. On the argument, it was contended by the defendants, that the verdict ought to be entered for them on each of the issues joined on the record; but as the main question between the parties turns on the the third issue, which involves the question whether the invention of the plaintiff is a manufacture within the intent and meaning of the statute of James—that is, whether it is or is not the subject matter of a patent—and as the determination of this issue in favour of one party or the other will render the decision of the other issues free from difficulty, the simplest way will be to apply ourselves in the first instance to that question. Now, in order to determine whether the improvement described in the patent is, or is not a manufacture within the statute, we must in the first place ascertain precisely what is the invention claimed by the plaintiff, and then, by the application of some principles admitted and acknowledged in the application of the law relating to patents, and by the authority of decided cases, determine the question in dispute between the parties. The plaintiff describes the object of his invention to be the application of anthracite or stone coal, combined with hot-air blast, in the smelting or manufacture of iron from iron stone, mine, or ore; and states distinctly and unequivocally at the end of his specification, that he does not claim the use of a hot-air blast, separately, as of his invention, when uncombined with the application of anthracite or stone coal; nor does he claim the application of anthracite or stone coal when uncombined with the using of hot-

air blast; but that what he claims as his invention, is the application of anthracite or stone coal, and culm, combined with the using of hot-air blast in the smelting and manufacture of iron from iron stone, mine, or ore. And the question therefore becomes this—whether, admitting the using of the hot-air blast to have been known before in the manufacture of iron with bituminous coal, and the use of anthracite and stone coal to have been known before in the manufacture of iron with cold-blast; but that the combination of the two together (the hot-blast and the anthracite) were not known to be combined before in the manufacture of iron, whether such combination can be the subject of a patent? We are of opinion that if the result produced by such a combination, is either a new article, or a better article, or a cheaper article to the public, than that produced before by the old method, that such combination is an invention or manufacture intended by the statute, and may well become the subject of a patent. Such an assumed state of facts falls clearly within the principle exemplified by Abbot, Chief Justice, in the case of the King *v.* Wheeler, 2nd Barnewall and Adolphus, 349, where he is determining what is, or what is not, the subject of a patent: namely, it may perhaps extend to a new process, to be carried on by known instruments, or elements, acting upon known substances, and ultimately producing some other known substance, but producing it in a cheaper or more expeditious manner, or a better or more useful kind. And it falls also, within the doctrine laid down by Lord Eldon, in Hill *v.* Thompson, in 3rd Merivale, 629: namely, there may be a valid patent for a known combination of materials previously in use for the same purpose, or even for a new method of applying such materials; but the specification must clearly express that it is in respect of such new combination or application. There are numerous instances of patents which have been granted where the invention consisted in no more than in the use of things already known, and acting with them in a manner already known, and producing effects already known; but producing those effects so as to be more economically or beneficially enjoyed by the public. It will be sufficient to refer to a few instances, some of which patents have failed on other grounds, but none on the ground that the invention itself was not the subject of a patent. We may first instance Hall's patent for applying the flame of gas to singe off the superfluous fibres of lace, when a flame of oil had been used before for the same purpose; Derosne's patent, in which the invention consisted in filtering the syrup of sugar through a filter, to act with animal charcoal, and charcoal from bituminous schistus, where charcoal had been used before in the filtering of almost every other liquor, except the syrup of sugar; Hill's patent, in 3rd Merivale above refered to, for improvements in the smelting and working of iron; there the invention consisted only in the use and application of the slags or cinders, thrown off by the operation of smelting, which had been previously considered useless, for the production of good and serviceable metal, by the admixture of mine

rubbish. Again, Daniel's patent was taken out for improvements in dressing woollen cloth, where the invention consisted in immersing a roll of cloth, manufactured in the usual manner, in hot water. (See the King *v.* Daniel, in Mr. Godson's book on patents, 274.) The only question, therefore, that ought to be considered on the evidence, is—was the iron produced by the combination of the hot-blast and the anthracite a better or a cheaper article than was produced from the combination of the hot-blast and the bituminous coal; and was the combination described in the specification new, as to the public use thereof, in England? And upon the first point, upon looking at the evidence in the cause, we think there is no doubt that the result of the combination of the hot-blast with the anthracite, on the yield of the furnaces, was more; the nature, property, and quality of the iron better; and the expense of making the iron less, than it was under the former process, by means of the combination of the hot-blast with the bituminous coal. It is to be observed, that no evidence was produced on the part of the defendants to meet that given by the plaintiff on these grounds, and that it was a necessary consequence, from the proof in the cause, that the substitution of the anthracite coal, in whole, or in part, instead of or in the place of, bituminous coal, from the substitution of that the manufacture of the iron should be obtained at less expense. It was objected, in the course of the argument, that the quality, or degree, of invention, was so small that it could not become the subject-matter of a patent; that a person who could procure a license to use the hot-air blast under Neilson's patent had a full right to apply that blast to coal of any nature whatevever, whether bituminous or stone coal. But we think if it were necessary to consider the labour, pains, expense, incurred by the plaintiff in bringing his discovery to perfection, that there is evidence in this cause that the expense was considerable, and the experiments numerous. But in point of law, the labour of thought, or experiments, and the expenditure of money, are not the essential grounds of consideration on which the question, whether the invention is or is not the subject-matter of a patent, ought to depend; for if the invention be new and useful to the public, it is not material whether it be the research of long experiments and profound research, or whether by some sudden and lucky thought or mere accidental discovery. The case of Monopolies, 11th Coke, states the law to be, that where a man, by his own charge or industry, or by his own wit or invention, brings a new trade into the realm, or any engine tending to the furtherance of a trade, that never was used before, and that was for the good of the realm, that the King may grant him the monopoly of a patent for a reasonable time. If the combination now under consideration be, as we think it is, a manufacture within the statute of James the First, there was abundant evidence in the cause that it had been the great object and desideratum, before the granting of the patent, to smelt iron stone by the means of anthracite coal, and that it had never been done before. There is no evidence on the part of the

defendants, to meet that which the plaintiff brought forward. These considerations, therefore, enable us to direct that the verdict ought to be entered for the plaintiff, on the third issue; that it was a new manufacture—new as to the public use and exercise thereof, within England and Wales. On the same ground, also, the second issue is disposed of in favour of the plaintiff. No evidence was produced on the part of the defendants, to show any inventor earlier than the plaintiff; nor does the fact that there was an earlier inventor appear from the cross-examination of the plaintiff's witnesses. As to the first issue—namely, whether the defendants had infringed the patent, we think it clearly appears on the evidence, that the defendants had used, either in part or in whole, the combination described in the specification of the plaintiff's patent; the plaintiff's evidence goes fully to show certain infringements, and that is not met by any explanations on the part of the defendants. Indeed, the defendants' case did not appear to rest on this point at the trial, so much as on the important question raised by them, whether the improvement described in the specification was a manufacture within the statute of James. Upon the fourth issue, which raised no more than the usual inquiry—whether the nature of the invention was sufficiently described in the specification?—the usual evidence was given that persons of competent skill and experience could, by following the direction, produce the manufacture described, with success; and this evidence was entirely unopposed. Upon this issue, also, the verdict ought to be entered for the plaintiff. With respect, however, to the issue raised in the rejoinder, in the plaintiff's replication to the fifth plea, we are of opinion, that taking the whole evidence brought forward by the plaintiff, it is impossible to perceive any substantial or real distinction between the hot-air blast, and the machinery and apparatus described in Neilson's specification, from that described or referred to in the plaintiff's; or to say that the using by the plaintiff of the invention described in his specification, was any other than a using and imitating of the invention described in Neilson's specification. The plaintiff, indeed, worked by license under Neilson's patent, at the time of his discovery. On this fifth issue, therefore, we think the verdict should be entered for the defendants. Then arises the question whether the plaintiff is or is not entitled to the judgment, notwithstanding the verdict on the fifth issue, on which point the argument on the part of the defendants is, that the taking out a patent for an invention, which invention cannot be used or enjoyed by the public except by means of the former invention of another person, which former invention is itself the subject-matter of a patent still in force, is void by law. Undoubtedly, if the second patent claims, as part of the invention described in it, that which had been the subject-matter of a patent then in force, it would be void, on the double ground that it claimed that which was not new (which, indeed, would equally be the case if the former patent had expired), and also that it would be an infringement of, and inconsistent with, a former grant of the King, still in force; which

latter consideration, alone, would make a new grant void. But in this case, there is an express disclaimer of any part of the invention to the use of the hot-air blast which was covered by Neilson's patent, the specification describing that the application of the hot-air blast was well understood, and extensively applied, in many places where ordinary fuel is employed. The validity, therefore, of the plaintiff's patent cannot be impeached on either of the grounds above adverted to. Unless, therefore, the grantee of the new letters patent is bound by law to specify whether such former invention, which is excepted, was so excepted on the ground of its being generally known and used by the public, and on the ground that it was the subject of a patent that secured the use of it to a former patentee, the new patent will be good; but that distinction is as much in the knowledge of the public as the grantee of the patent. If, indeed the new patent had been taken out for improvement, or alterations, in an invention secured by a former patent, then, for obvious reasons, greater particularity would be necessary to distinguish the new from the old. But the present specification expressly says—"I take the whole of the invention, already well known to the public, and I combine it with something else." Now, it is further argued, that in point of law no patent can be taken out which includes the subject-matter of a patent still remaining, or in force. No authority was cited to support this proposition; and the case which was before Lord Tenterden, and in which he held that where an action was brought for improvements in a former patent granted to another person, and still in force, that the plaintiff must produce the former patent and specification; *that* at least affords a strong inference that the second patent was good. Lewis *v.* Davis 3rd Carrington, and Payne and Harmer *v.* Payne, 11th East., are clear authorities on the same point; and upon reason and principle there appears to be no objection. The new patent, after the expiration of the old one, will be free from every objection; and whilst the former exists, the new patent can be legally used by the public by procuring a license from Neilson, or by purchasing the apparatus from him or some of his agents; and the probability of the refusal of a license to any one applying for it is so extremely remote that it cannot enter into consideration as a ground of legal objection. On the whole, therefore, we think the verdict is to be entered for the plaintiff on all the issues except the fifth; that the verdict is to be entered for the defendants on the fifth issue—but that, notwithstanding such verdict, the judgment must be given for the plaintiff.

Mr. M. Smith :—My Lord, there are certain certificates which were to be given, in case the judgment was for the plaintiff.

The Lord Chief Justice :—You must apply to me for those ; that is not a matter for the Court—that is a matter for myself.

A dangerous Property of Wood Ashes exposed, and some of their other Properties examined. By Dr. JOHN T. PLUMMER, of Richmond, Indiana.*

I HAVE recently devoted some time to a further examination into the properties of wood ashes, and especially into that property by which heat is conveyed from a small space on their surface deeply into the interior of the largest masses. I consider the subject of sufficient importance to claim the attention of the general as well as the scientific reader; for I cannot forbear thinking, that at least some of the mysterious conflagrations which are repeatedly occurring, are chargeable to this hitherto unsuspected cause.

Judging by the remarks of Prof. Hubbard, accompanying two cases of combustion in wood ashes, reported by him in a late number of this journal, it appears to be his opinion that the caloric in question was generated within and near the bottom of the heap, by a spontaneous but unknown process. I conceive that the following experiments render this opinion highly improbable, and they go to sustain the view taken by the present writer, so far as it respects the origin of the caloric, and perhaps measurably as it regards the means by which the heat is diffused throughout the ashes. They show that the heat-retaining power is not peculiar to ashes, but is common to various pulverulent substances; that this residue of combustion contains an appreciable quantity of charcoal in a state of minute division; and, as formerly stated, that it is unsafe to deposit hot ashes upon, perhaps, the largest heaps of cold ashes. I shall marshal these experiments under the head of—

Ignitibility of Wood Ashes.—1. A pint of sifted ashes was made into a conical heap four inches high, upon a folded newspaper, and a coal lighted at one corner only, was laid upon the summit and very slightly covered. In seventeen minutes the coal was examined and found to be wholly ignited. It was again covered, and in eleven minutes afterward that part of the paper on which the ashes rested became quite warm, and also the board beneath it. On sliding the paper nearly off the board, and gently bending it convexly upwards, I succeeded in producing a fissure, extending from the apex of the cone downward to a considerable depth. By this means I was enabled to see the interior of my diminutive volcano, and to discover that the ashes within were *red hot*, if not incandescent, as far down as the fissure extended. After this peep, I closed up the crater by sliding the paper back upon the board, and waited an hour from the beginning of the experiment. At the expiration of this period, the coal was not wholly consumed, and the ashes were still quite warm.

The coal used in the foregoing instance was of sugar-tree wood, and at the time it was placed upon the ashes, two other coals, one of

* From "The American Journal of Science and Arts."

sugar-tree and the other of beech, were thoroughly ignited and laid upon a board. In two minutes the "fire went out" of both these coals.

2. A wooden pill-box of the largest size was filled with sifted ashes, and an oak coal weighing seven grains was barely buried in them. In thirty-five minutes the box was very warm all over; and at this time I surrounded it with cold ashes. In twenty minutes more, the ashes within and immediately around the box were uncomfortably hot.

3. I renewed the second experiment, with the exception of not wholly covering the box. The edge was left exposed, to ascertain whether it would not act as a vent to the accumulating caloric. In half an hour I examined the coal, and found it extinct and the ashes cold. The coal in this case was of beech.

4. This beech coal lighted at one corner, was placed on a cone of sifted ashes, as in the first experiment, and in twenty minutes it was thoroughly ignited. I now pressed a cylinder of paste-board perpendicularly into the ashes, so as to include the coal and most of the heated ashes. The upper edge of the cylinder was left uncovered. I did not examine the coal for an hour; it was at that time not consumed but dead, and the ashes were entirely cold.

5. I built a cone of a quart of pale ashes, and deposited eight or ten dead coals some distance apart, near the base and remote from the surface; at the apex I buried a live coal as before. In three quarters of an hour, stiff paper or a splinter of wood thrust into the centre of the heap took fire; and on demolishing the pile, I found that the heat had descended to the coals below, and ignited them; indeed they were partially consumed, and the whole interior of the base of the cone was extremely hot.

6. A wooden box ten inches deep and eleven inches square, was filled with unsifted ashes as cold as an exposure of several weeks in winter could make them. A pint of hot ashes was thrown upon the middle of the surface and left uncovered. In eight hours all the central portion of the ashes was hot enough to fire wood thrust into it, and two sides of the box were incalescent. In twenty-three hours the bottom of the box was quite warm, the top of the ashes cool, and the sides of the box were becoming cool. A stick plunged to the bottom of the ashes, was drawn out ignited or burnt at the end, but not even charred above it. In thirty hours the bottom of the box was almost insupportably hot; and the upper half of the ashes retained but little heat. In thirty-six hours, the temperature of the ashes being much reduced, I emptied the box, and found the bottom of it on the inside near the middle converted to coal, one of the sides considerably charred, and another browned by the heat. Coals were found in different parts of the ashes, but I believe they were confined to those portions through which the heat did not travel.

The ashes used in the foregoing and the subsequent experiments, were derived from the mixed combustion of hickory, beech, sugar-tree, oak, and a few other kinds of wood; and the sieve employed

consisted of twenty-four by thirty-two interstices to the square inch.

To what cause could I attribute the augmentation of heat and its downward course, which the preceding instances exhibit? The plausible answer was carbon. There, said the spirit of conjecture, was the fire, burning its way into the ashes, and leaving successive portions of them to cool after it had consumed the combustible matter out of them; travelling downward like the Goth's descent upon Rome, into regions where its fierceness could be fed. There, too, was the *gray* colour of the ashes, produced, said conjecture, by the admixture of fine carbonaceous particles with the pure white cineritious matter. To prove that the proper colour of wood ashes is white, there lay the beautiful specimen with gossamer lightness upon the hearth, the residue of the undisturbed combustion of a solitary ember; showing the delicate fibrous structure of the original wood; with open avenues on every side, and a thousand apertures within for the free admission of atmospheric oxygen to every atom of carbon; the carbon thus affianced to oxygen had escaped into the air, leaving its white mansion unshaded by its presence. And how could I better account for the various shades of gray which ashes present, than by supposing them to arise from the various proportions of the black powder intermixed? And then, there were the uniform results of repeated trials by fire, in which something escaped out of the contents of the crucible; and what could this be but carbon? Such was the language of imagination before experiment had fully uttered its voice. To strengthen these conclusions, I applied myself to other evidences; but these, to my disappointment, instead of supporting, kicked against my imaginings.

7. Selecting magnesia as an article possessing physical properties somewhat similar to those of ashes, I erected a cone of this material, and at the summit buried a partially ignited coal. In a few minutes I was surprised to find the whole coal was alive with fire. Shortly afterwards the magnesia beneath the coal became ignited, and the bottom of the heap almost intolerably hot.

8. Guided by the specific gravity and the compressibility of the substances employed, I repeated the experiment with pulverized chalk instead of magnesia. The chalk soon became red hot beneath the coal, and the base of the heap heated beyond endurance.

Thus discovering that these alkaline earths possessed the same heat-preserving properties as ashes, and that the same downward centralizing tendency of caloric was shown in all, I was led to the conclusion that the heat eliminated and diffused in the sifted ashes was the result of the combustion of the single coal buried in them; and considering their low conducting and radiating power, it appeared probable that the amount of heat apparent was not very far from the absolute quantity generated during the combustion. In every instance, while the central parts of the cones were red hot, the exterior of the ashes, except at the apex, was cold throughout the experiment. The caloric is evolved faster than it is diffused, and of course it accumulates within a small sphere near the coal to an ig-

niting temperature; combustible matter lying at the circumference of this sphere would ignite and generate another ball of fire, and this produce another, and so on indefinitely, or while the last ignited spheres reached new combustible matter. In this manner I conceive the caloric travelled in the fifth and sixth experiments, and I see no reason why it should not under similar circumstances circulate through a bed of ashes spread over the whole earth.

Satisfying myself in this manner that the presence of pulverulent charcoal was not essential to the phenomenon in question, I submitted other powders to similar trials.

9. Fine sand, scorified wood ashes, anhydrous sulphate of lime, common earth, all thoroughly dried, and the earth and sulphate reduced to subtle powders, were severally made the tenements of a fully ignited coal; but in spite of all the persuasion I could command, the coal refused to be buried alive in such sepulchres as these; almost as soon as it was decently interred it expired.

During my inquiries into this subject, I was induced to compare the physical and other properties of various powders. Omitting my tables of their specific gravity, porosity, &c., as incomplete, I give the other results below. The substances were dried at a high temperature, and passed through the same sieve; the force used in ascertaining the compressibility was sixty pounds.

The *gravity* was determined by weighing a given measure, without compression or jar.

Water, being	1000
Magnesia, was	164
Ashes	356
Chalk	582
Scorified wood ashes	910
Plaster of Paris	921
Common earth	1035
Sand	1358

Compressibility.

The loose measure, being	100
Ashes were reduced in bulk, to	25
Magnesia	50
Chalk	50
Plaster of Paris	69
Common earth	69
Scorified ashes	86
Sand	88

The sand could be shaken into a less space than it could be pressed by the force employed.

Conductibility.—I selected neat paper pill-boxes of uniform size, colour, &c., and filled them evenly and without jarring, with the several powders tested. On the centre of the surface, I carefully placed very small squares of tinfoil supporting a particle of phosphorus. Thus prepared, the boxes were at the same instant care-

fully set upon an equally heated metallic plate, and the time of fusion and of deflagration noted. I give, in the subjoined table, the average results of several trials, in seconds:

Magnesia.	Ashes.	Scorif. ashes.	Chalk.	Sand.	Coal.	Earth.	Gypsum.	Sugar.
42·335	112·267	125·242	123·293	125·242	77·420	182·300	154·314	218·407

When fused, the phosphorus assumed a hemispherical form, and delicate vivid lines shot like lightning from the margin apparently across the semiglobules, and continued thus to play in the most lively manner from various points in rapid succession until inflammation ensued. This pretty miniature pyrotechny can only be seen in the dark.

Hygrometric Power of Wood Ashes:—One hundred grains of dried ashes were lightly spread over an area of sixty square inches, and were exposed with due caution in the shade for twenty-four hours, when the noon temperature was 62°, and the air clear and calm. In this time they gained no weight; coutinued exposure for seventy-two hours enabled them to gain ·55 of a grain. But when the temperature was near the freezing point at noon, they frequently gained in eight to twenty-four hours from ·66 to 1·66 of a grain. A fine sponge was converted into a sensitive hygrometer by saturating it with a solution of carbonate of potash, and drying it at 170°. Thus prepared the sponge gained from thirteen to forty-four per cent. more than the ashes, and it assisted me in making out the following deductions from eighteen experiments on the hygrometric properties of ashes. 1. They absorb atmospheric vapour more rapidly at a low than at a high temperature. 2. They do not cease to act hygrometrically at a temperature considerably below the freezing point. 3. The range of per centage of increase is from 0 to 1·66. 4. Different parcels of ashes possess different degrees of hygrometric power. This is owing perhaps to the varying quantity of potash present.

Carbonaceous Dust in Ashes.—It became interesting to ascertain how much, if any carbon, in a pulverised state, existed in ashes. For this purpose I employed sundry rather rude methods, now to be mentioned. 1. By pouring a sufficient quantity of suphuric acid on sifted ashes, to moisten them, much caloric was disengaged, and a white paste formed, in which black particles were very obvious. These particles washed and crushed between the teeth, produced the peculiar sensation of breaking charcoal. On diluting this paste with a large quantity of water, a milky mixture (sulphate of lime) was obtained, in which the black particles rapidly subsided. All the black sediment, however, was not coal. 2. One thousand grains of sifted ashes were stirred in a large quantity of water, and allowed to subside. In a short time the insoluble parts settled at the bottom, and most of the coal with them, very few particles of it floating on the water. Hence the porosity of the coal must have been sufficiently destroyed to render the fragments specifically heavier than this fluid. 3. The washed ashes used in the foregoing experiments, were examined by a

microscope, and particles of coal clearly seen in them. Specimens of well burnt and sifted ashes from the stove, exhibited the same appearance. 4. Several hundred grains of sifted ashes were treated with nitric acid, and after long digestion, the residue was washed, dried, and weighed; the quantity was six per cent. of the original weight of the ashes. Under the microscope this residue was found to consist of particles of coal, a greater proportion of black vitrified grains, and transparent particles which scratched glass, and appeared to have undergone partial fusion. 5. One hundred grains of sifted ashes were in like manner submitted to the action of hydrochloric acid. On diluting the solution with water, a black matter was immediately deposited with gray particles beneath it. The black sediment, washed and dried, assumed a black-brown colour, and weighed a fraction over six grains. This powder scintillated in the blaze of a candle like coal-dust; under the action of the blowpipe it became gray, but the principal part remained unconsumed.

Having by these means convinced myself of the presence of coal-dust in ashes, it followed that an ordinary fire did not always consume all the carbon of wood; and to arrive at an approximation to the degree of heat necessary to burn it out of ashes, I tried the next experiments. 6. Sifted ashes were pressed firmly into a crucible, and to expel moisture I subjected them to a temperature of at least 440° for an hour. The temperature was ascertained by the fusion of tin. The ashes were then weighed, and subjected to a red heat for half an hour. On weighing again, there was no appreciable loss. Sulphuric acid and the microscope detected, as before, particles of coal in these *crucibulated* ashes. 7. Most of the ashes of the last experiment were placed loosely in the crucible, and again heated to redness for half an hour. The result was as before: no loss of weight. 8. Intimately mixing thirteen grains of coal-dust with four hundred and sixty-seven grains of the crucibulated ashes of the seventh experiment, and pressing them together, they were heated to redness for a considerable time. In this case there could be no doubt of the presence of carbon; yet after cooling, the mixture weighed just four hundred and eighty grains, and consequently had suffered no ponderable loss. 9. I now placed the crucible as it came from the furnace in the eighth experiment in a smith's forge, and heated it to incandescence for several minutes. At this heat the ashes lost nine grains, leaving at least four grains of the adventitious carbon unconsumed. 10. Four hundred and eighty grains of sifted ashes, hot from the stove, were put into the crucible, and exposed to the highest heat of a smith's forge for twenty minutes. On cooling, they weighed only four hundred and thirty-one grains, having sustained a loss of forty-nine grains! The ashes in the middle of the surface were gray, but all other parts throughout were bluish brown, or blackish and brown. The mass was porous, considerably contracted, and cracked through the centre nearly to the bottom of the crucible; it crumbled under considerable pressure, but retained its form in water, yielding up its soluble parts without falling to pieces.

Throughout the slag were scattered grains of a beautiful cerulean hue, insoluble in nitric or sulphuric acid, and exhibiting under the microscope a botryoidal surface. These grains were evidently the product of the intense heat of the forge; and if we can suppose a sufficient quantity of alumine present in the ashes, what forbids them being *hauyne* of domestic manufacture?

During this last crucibulation, care was taken, as before, to prevent the escape of ashes; the crucible was kept erect and well covered with a steel plate, and every precaution used to avoid error. Add to this vigilance, the fact that the ashes did not occupy more than one-third the depth of the crucible, and we can hardly conceive that reverberating currents of air from the bellows could dissipate any of the ashes. But to determine whether the loss was attributable to such an accident, or to the loose state of the ashes, I tried experiment 11. Seven hundred and twenty-nine grains of hot, sifted ashes, were pressed into the crucible, and carefully heated in the forge for fifteen minutes. The loss was seventy grains, or 9·6+ per cent. The ashes were scorified, and presented the *hauynoid* grains and every other appearance and property of the slag of the tenth experiment. If no ashes escaped, how shall we account for the loss of weight? Is there a ponderable element in ashes, which has eluded former analyses? Or did the intense heat of the forge decompose some of the constituents of ashes, drive off the gaseous elements, evaporate water of composition, and volatilize any of the solids? Regarding the known constituents of ashes, I ascertained that in one specimen of the scorified ashes there was a trace of caustic lime; I detected it in no other portion of the slag. Water in which the scorified ashes had digested several days, produced a very faint white cloud, on the addition of oxalate of ammonia. Another portion of the water boiled down exhibited a lively effervescence on the addition of an acid. The slag from the crucible, though exposed to a damp atmosphere for several days, showed no disposition to deliquesce. The long digested and thoroughly washed scorified ashes produced abundant effervescence with nitric acid; the resulting nitrate had the peculiar bitterness of that salt of lime, and produced a very copious white precipitate with oxalate of ammonia. I judge, therefore, that but a very small quantity of carbonic acid was driven off from the carbonates of lime and of potash; not more perhaps than would be counterbalanced by the oxidation of the iron contained in ashes. The presence of silica, iron, and a sulphate, were satisfactorily shown; an accident prevented me from testing the presence of a phosphate or a chloride. It therefore remains with me a moot question, what occasioned the loss in the tenth and eleventh experiments?

Since the publication of my former communication on this subject, I have received information of other accidents similar to those reported to this Journal, where large quantities of stored ashes had become extensively heated, and sometimes the wooden repository inflamed. In several instances it was known that hot ashes had been thrown upon the heap, under the impression that there was as much

safety in doing so, as in depositing them upon the ground remote from combustible matter. What part the small portion of *pulverulent* coal may perform in the propagation of the fire, I am not prepared to say. That coal in some degree of comminution is necessary to the passage of caloric from one part of the mass to another I am convinced; and that the charcoal in some state of division exists in most wood ashes, is easily proved by the use of a coarse sieve, to say nothing of the large coals generally apparent, and especially as they float upon the water of the leech-tub. It may here be asked, if this combustible material is thus distributed through the ashes, why does not the fire always diffuse itself equally in the heap, instead of pursuing a devious path through it? What contingencies may direct its course, I do not know; but nothing more happens here than happens to a piece of paper inflamed at its edge; it seldom consumes equally, but the combustion proceeds in very uncertain directions.

NEW PATENTS, &c.

Specification of a patent granted to Arthur Wall, *of Shadwell, Great Britain, for a composition for preventing Corrosion in Metals, June 22nd,* 1841.

Be it known that I Arthur Wall, of Shadwell, in the county of Middlesex, in that part of the kingdom of Great Britain, called England, surgeon, have invented a new composition for the prevention of corrosion in metals; and I do hereby declare that the following is a full and exact description of it. To enable others skilled in the art to use my invention, I will proceed to describe the mode of manufacturing the same, and the application thereof. I place twenty pounds of the strongest muriatic acid, diluted with three gallons of water, in a shallow pan, or vessel, made of cast iron. I then take 112 pounds of filings of either steel or bar iron, or other wrought iron; I heat them to redness and throw them into the mixture of acid and water, for the purpose of oxidizing the filings. I then place the pan on a sand bath (heated by a flue from a furnace), which digests the filings, and facilitates the oxidation. I repeatedly stir up the whole, and after subjecting them to this process for about twenty-four hours, or until ebullition takes place, and the greater part of the filings are taken up by the liquor, or mixture, I allow the oxides thus obtained to run off through a tap into a vessel beneath, leaving the metal not operated upon at the bottom. When these oxides are quite settled, the clear mixture or liquor is run off from them into a third vessel, and then the filings must be subjected to the same process in the original mixture to complete the oxidation, (that is) they must be again made red hot, and the mixture which has run into the third vessel thrown upon them, and this process must be repeated until all the filings have oxidized that can be made

to do so. The oxides thus obtained I now expose on an iron plate, made red hot over a furnace, until all moisture has evaporated from them, and they assume a red appearance. I then mix with them sixteen pounds of quicksilver, by sifting it through a very fine wire sieve on to the oxides, and afterwards I intimately mix it with them by rubbing the whole down in a mortar, or other suitable process, and when so mixed I then add as much water as will cover the surface, and from eight to nine pounds of strong nitric, or nitrous, acid, and again place the whole on the furnace plate, or iron bath, and repeatedly stir it until all the menstrum, or liquor, has nearly, or completely, evaporated. I then place the whole mass in a mortar, or other pounding machine, and bray or pound it until it is in a complete state of blackness. I then mix it with water, and stir or wash it until all the light particles are washed out. I then allow it to settle, and when the settlement has taken place, the water is poured off from the sediment at the bottom. This sediment I then place in a crucible, or earthen retort, with a receiver attached, adapted for the reception of any chloride or mercury that may escape or come over (the contents of this receiver I preserve, in order to re-add to the general mass afterwards, when cool); then I make it red hot, and when in this state I plunge it into fresh boiling water, and stir it for a few minutes, and then allow it to settle. I then pour the water off, let it cool, and add the chloride as before stated; and after the last mentioned process I add to it one quarter of its own weight of common black lead, or minium, commonly called "red lead," according to the colour which the operator wishes the composition to assume. Previously to applying this composition to metal, I add to it such a quantity of boiled linseed oil and spirit of turpentine (in the proportion of one-fifth of of spirit of turpentine, to the oil used), as will reduce it to a state sufficiently liquid to be spread with a brush. This preparation I then apply as thinly as possible, by means of a brush, to sheets of copper or other metal; which sheets I afterwards subject to a heat gradually raised to about 300 degrees of Fahrenheit's thermometer, so as to make the metal imbibe the preparation: this heat must be applied to the prepared sheets of metal, without smoke or flame, by placing the sheets on trucks in contact with the flue plates, in the manner hereinafter described. The mode of applying this heat may be various, but in order the more distinctly to explain my meaning, and likewise my mode of operating, I shall now proceed to describe the furnace which I use, and find to answer the purpose. I erect two or more horizontal flues, the construction and dimensions of which may be varied according to circumstances, which flues should gradually decline towards the extreme end from the furnace bars, so as to produce a good draught and communicate a stronger heat to the plates above next mentioned. The flues I cover with cast iron plates. I then raise the exterior walls of the furnace to the height of from about three to six feet above the iron plates, which walls

must be bound with iron braces to prevent them from cracking from the excessive heat. I then place thin sheets of iron slightly curved, thus forming a roof, and rest them on the exterior wall. Each end of the chamber (thus formed) is closed by an iron plate, made to slide up and down by a pulley, so as to act as a damper, and let out or confine the heat. The heat from the flues is carried away by a common chimney, which has a damper in it for the purpose of controling the heat. The sheets of metal prepared with the composition as above described, are thus placed upon iron trucks, between upright pins which run on wheels of four inches in diameter, and are thus placed over the iron plates made hot by the flues; the heat must be gradually applied to prevent the composition from blistering on the metal, by the trucks being first placed at the extreme end from the furnace bars and gradually rolled over the flue till evaporation ceases, and the metal assumes a dark appearance: this completes the operation. When preparing iron tanks with the composition, I apply the furnace heat merely sufficient at first to expel the moisture from the metal, and when in that state I take them out and sprinkle over them as much charcoal, very finely pounded, as will be absorbed by the metal, which gives to the metal, when prepared, a glossy appearance. I then apply the stronger heat, and the operation is completed.

Now, I do not claim as any part of my said invention any of the separate processes, or the use of any vessels or furnaces. But what I do claim as my invention, and desire to secure by letters patent, is the composition prepared as above described, for the prevention of corrosion in metals and for other purposes.

Specification of the Patent granted to Henry Browne, *of Codnor Park Iron Works, in the County of Derby, Iron Manufacturer, for Improvements in the Manufacture of Steel. Sealed April* 22, 1841.*

To all to whom these presents shall come, &c., &c. My invention relates to a mode of manufacturing steel from iron, by obtaining the iron in a fine granulated state, and then treating it with cementation with carbon, as hereafter described; and in order that my invention may be fully understood, and readily carried into effect, I will proceed to describe the means pursued by me. In working according to my invention, the crude, or pig, or refined metal, is to be treated as if about to be made into bar or malleable iron, by the purifying and decarbonizing process of puddling: that is, by stirring the melted mass in the furnace with iron tools, and exposing it to the action of the heated air, as usually practised, until the metal becomes in a dry granulated state, all of which is well understood by puddlers; and in place of carrying on the pro-

* "Report of Patent Inventions."

cess further in the puddling furnace, the iron, in the granulated state, is to be removed from the furnace, and when cold, a large proportion may be passed through sieves, the meshes of which are about twenty in an inch, and the remainder may be crushed or ground, until the grains will pass through the same sieve, or the various sized grains may be separated by various sieves, differing in the size of the mesh according to the will of the operator; but I consider that the smaller the grains, the more advantageously will the process of manufacturing steel therefrom be carried on; and it is the converting of granulated iron, such as above described, into steel, by cementation with carbon, that constitutes my invention.

The granulated iron is next submitted to cementation, which I perform in the following manner:—I use an ordinary cementing, or converting furnace, the nature of which is well known, and the cementing chests, or what are usually called pots, are about ten feet long, three feet wide, and three feet deep; but the dimensions may be varied. I prepare a number of frames of iron or wood—I prefer wood, and that which is called pine—an inch or two less in length and breadth, so that they will pass easily into the pots; the frame, made of wood, about a quarter of an inch thick, and one inch deep, and divided by wood partitions, at distances of about every ten or twelve inches. The carbon I prefer to use is wood charcoal, crushed and passed through a sieve, the meshes of which may be about one quarter of an inch square, though other carbon may be used. I place a quantity of the charcoal, to the extent of about half an inch in thickness, over the bottom of the pot; and this I cover with paper, or other suitable substance, on which I place one of the wood frames, and fill the compartments thereof with granulated iron, before described. On the surface of the granulated iron I place another covering of paper, and apply more charcoal, and press it into all the spaces between the frames and the sides of the pot; and I cover the paper evenly to the extent of half an inch; I then apply another thickness of paper, then another frame, which I fill with granulated iron, as before, and cover it with paper; then charcoal, then paper, and then another frame; and so on until the pot is full, having charcoal on the top to the extent of three or four inches deep; I then cover the whole close down with loam sand, or "swarf," from a cutler's grinding-mill, or other suitable substance, to exclude the air, tempered and spread over the top of the pot to the thickness of five or six inches. I now heat the furnace to a high heat, as is well understood by steel manufacturers, and let the pot and its contents remain at that heat for a sufficient time, which I find to be from thirty to sixty hours. The time and heat may be varied, according to the state of carbonization required. The pot and its contents are to remain until cool, and when opened the steel will be formed into cakes of the size of the compartments in the frames, and may be removed; the charcoal and paper may be brushed off. The steel thus produced is then broken into pieces,

and melted in the usual manner, in crucibles. The high or low state of carbonization may be judged of by the colour, which will vary from blue to purple, straw colour, and grey; that which is blue having less carbon, and is less firmly united together; and that which is grey, and more united, is charged in a higher degree with carbon. The quantity and hardness of the steel may be varied by a judicious selection of the cemented steel, so as to adapt it to the purposes to which it has to be applied.

Specification of the Patent granted to JOHN BETHELL, *of Saint John's Hill, Wandsworth, in the county of Surrey, for Improvements in Treating and Preparing certain Oils and Fatty Matters. Sealed March* 28, 1840.*

To all to whom these presents shall come, &c., &c. The object of my invention, so far as relates to improvements in treating and preparing oils, is to render certain oils, viz.: whale, elephant whale, Newfoundland whale, seal, rapeseed, teel, olive, palm, cocoa-nut, or any of the other common oils, more useful, either for lubricating machinery, or for the purpose of illumination, and which I effect, first, by separating, clarifying, or precipitating, a portion of the gelatinous, albuminous, or other matters contained therein; and, secondly (when such are required for burning in lamps, or for illumination), by adding thereto a portion of hydrocarbon, or essential oils, hereinafter named; and as regards the treating and preparing of certain fatty matters, the object of my invention is the manner in which I have hereinafter described, from butter of palm, cocoa-nut oil, or any other vegetable concrete oil, produced an oil which is more useful for mixing my purified oils, and which process also improves the fatty matter, or concrete oil, so operated upon, and which improvements I propose to carry into effect, in manner hereinafter described; that is to say—as to that part of the invention which relates to oils:—

First Process.—I take any or either of the common oils above named, and I purify them from the gelatinous, albuminous, and other matters contained therein, by first thoroughly well mixing the oil with a solution of tannin, which may be obtained from any of the vegetable matters yielding it; but I prefer using a strong infusion of gall-nuts in hot water, of which I take ten gallons, and thoroughly mix it with 100 gallons of oil, in any convenient manner. This mixture must afterwards be allowed to rest for three or four days, until all the tannin infusion, and precipitated matter, has settled down to the bottom. The clear supernatent oil is then drawn off, and again agitated and mixed with a solution of either acetate of lead, acetate of alumine, or sulphate of zinc, which I prefer using in the following proportions—viz.: 1 lb. of acetate of lead dissolved in 6 gallons of water, or 1 lb. of acetate of alumine dissolved in 4 gal-

* "Report of Patent Inventions."

lons of water, or 1 lb. of sulphate of zinc dissolved in 6 gallons of water; and I mix 10 gallons of either of such solutions with 100 gallons of oil; but I do not confine myself to these proportions, as solutions of different strengths can be advantageously used for different oils. The oil, after three or four days' rest, is drawn off from the top, and, if not sufficiently clear, must be filtered through oil-bags in the usual manner. During the period that the oil is undergoing the above operations, I prefer that it be kept at a temperature as near 17° Fahrenheit as possible. Should the oil be afterwards found to contain too much water, I cause it to be agitated with about 10 per cent. of fresh calcined sulphate of lime, in fine powder, or well dried carbonate of soda, to abstract the greater part of the water from it. The sulphate of lime, or soda, must be allowed to precipitate by rest, or the oil must be filtered through bags.

Second Process.—I take the oil as purified by my first process, or I take the more fluid parts of cocoa-nut or palm-oil, and for the purpose of making a good burning lamp-oil, I add thereto from 5 to 10 per cent. of either of the following essential oils, or hydrocarbons —viz.: petroleum, or rock-oil, Persian naptha, fine oil of turpentine, or the best essential oil, obtained from the distillation of coal-tar, or the oil obtained, as hereinafter described, by distilling any of the above essential oils with palm or cocoa-nut oil; the quantity to be added depends on the kind of oil operated upon, and upon its strength, and will vary between 5 and 10 per cent., but will be easily ascertained by trying a small sample of the oil first. The essential oil, or hydrocarbon, must be intimately combined with the oil, either by thorough agitation together, or by passing the vapour of the essential oil, or hydrocarbon, into the oil in an apparatus similar to a Wolfe's apparatus, but the former method I prefer for general use. In some cases, either from the oils operated upon being of a superior quality, or from so fine an article not being required, it is not necessary to use both the above processes, as either of them will be found sufficient for the purpose required. If a superior burning lamp-oil is required, I prefer using both of the above processes; but for a common lamp-oil, the oil may be prepared by either one of the above processes, without being submitted to the other, particularly when it is prepared by the second process only; and for a lubricating oil the first process only is used. As to the treating and preparing fatty matters, I take the "butter of palm," or "rough palm-oil," or "rough cocoa-nut oil," or any other concrete vegetable oil, and add to either of these 20 per cent. of either of the essential oils above named. Put it into a common still, and distil off the essential oil, and the volatile matter which rises from the palm, or cocoa-nut oil; but I prefer distilling with steam, and for that purpose I put the mixture into a close wood-vat, furnished with a steam pipe, leading from a steam boiler, and branching out into several other pipes, placed in the bottom of the vat, and pierced with small holes; the charging-hole of the vat being shut, steam is driven through the

mass, and the volatile products are conducted through a pipe fixed in the top of the vat, to a common distilling-worm placed in cold water. The volatile oil so condensed I mix with the oils for burning in lamps, as above mentioned, and the concrete fatty matter remaining in the vat is run out into casks, and will be found much improved, and more useful for many purposes.

In describing my improvements, I have stated the proportions of the different materials to be used which I prefer; but I do not confine myself to such proportions, as they may be advantageously varied with different oils.

Specification of a Patent granted to ALEXANDER PARKES, *of Birmingham, Artist, for certain Improvements in the production of Works of Art, by Electric Deposition. September* 29, 1841.*

THESE improvements consist in manufacturing articles in gold or silver by means of electric deposition upon suitable moulds, and in subsequently strengthening the articles so produced.

For this purpose the patentee uses the following solutions of these metals:—

Gold.—An ounce of pure gold is dissolved in aqua regia, and evaporated to dryness, when 2 gallons of water, and 16 ounces of prussiate of potash, are added to it. This solution is used at a temperature of 120°, or 130°, of Fahr.

Silver.—An ounce of pure silver, dissolved in nitric acid, is precipitated as oxide of silver, by lime water; the oxide being well washed, is mixed with one pound of prussiate of potash in two gallons of water.

The moulds used for this purpose are of metal, or other suitable material, in one or more parts, and may be removed from the finished article by melting or dissolving them.

The patentee prefers to use the compound, or independent battery, from which the electric current is conveyed from the battery into a cell, or vessel, containing the metallic solution, and a plate of gold or silver, to be eroded by the electric action.

If the metal is to be precipitated on the interior of a mould, as in forming a bust, &c., the plate is placed within the mould; but if the metal is to be deposited on the external surface of the mould, the plate is placed on the outside thereof.

If the article produced requires greater strength than would be desirable to be given by the thickness of the precious metals, it may be strengthened by depositing copper within it, until the required substance is obtained; or the article may be filled with some fusible metal.

The claim is 1. To the mode of manufacturing articles in gold and silver, by deposition thereof by electric agency, in or on suitable moulds or models, which may be removed from the articles

* "Mechanics' Magazine."

of gold or silver, when the same have been formed. 2. To the mode of manufacturing articles of gold or silver, on or in metal moulds or models, which are deposited by electric means; such moulds or models being afterwards removed as described. 3. To the mode of manufacturing articles of gold or silver, by electric deposition, on or in moulds or models, when such moulds or models are removed by heat or solution. 4. To the mode of manufacturing articles of gold or silver, by electric deposition, in or on moulds or models made up of parts. 5. To the mode of strengthening articles of gold or silver, produced in or on moulds, by electric depositions, by introducing a baser metal within them.

For an Improved Mode of Block Printing on various kinds of Fabrics, and for Apparatus, &c., for that purpose; Robert Hampson, *Manchester, June* 7, 1841.

The printing block is attached to a cross frame, which slides on straight edges at the four corners of the main frame, and has a rod attached to its middle that slides through a hole made in a set of arms at the top of a main frame. To the upper end of the rod there is a band attached, which passes over two pulleys, and has appended to it a counter weight, to balance the whole.

The fabric to be printed is drawn through the machine, and over the bed on which it rests, to receive the impression, which is given by means of blocks, in a manner well known to machinists. The colours are so arranged in separate sieves as that they can be drawn apart to receive new colours, and then brought together again to apply the colours to the block; the sets of sieves are placed upon a platform provided with wheels, running on rails. The platform, with the colours, is passed under the block, which is then let down to receive that which is wanted. The sieve is then drawn away, and the block descends upon the fabric, to impress the pattern; and the operation is in this manner repeated.

Claim.—"I declare that I claim as of my invention the apparatus, or machinery, constructed as set forth, for printing with blocks on woven fabrics of various kinds: that is to say, I claim the combining of a block for printing in various colours with the apparatus, or mechanical agents for causing said block to descend in a perpendicular direction in order to take up the colour from the sieve, or sieves, and to impress the pattern, or design, upon the fabric; said apparatus, or mechanical agents consisting of the cross frame, the arms, the straight edges at the corners of the main frame, the rod, the band, and counter weight, with their appendages; the whole being so connected as that by raising the counter weight the cross frame and block will descend, and the colour be received on the block, or the impression given to the fabric; and I claim the same however the same mechanical agents and contrivances may be modified or varied in size, form, proportion, or other particulars, not

departing from the principle of my invention; but I do not claim as my invention such mechanical agents, or contrivances, separately, but only as combined to effect the purpose aforesaid; nor do I claim as part of my said invention the apparatus hereinbefore described for traversing the colouring apparatus to and fro, by means of a railway carriage running thereon—the same although invented by me, having been by me used and practised previous to the grant of the said letters patent; nor do I claim as of my invention the arrangement of rollers and connected apparatus for causing the fabric to traverse, or be drawn over, the printing table, and conveyed away when printed. And I further declare that I claim as of my invention such colouring apparatus as hereinbefore described, in which several sieves are separated and held apart, to admit of the several colours being distributed and traced, or spread thereon, without admixture or interference one with another, and whereby the sieves are afterwards closed, or brought into juxtaposition, in order to the colours being taken up by the block, so as to produce at one impression of the block, or pattern, or design, or parts of a pattern or design, in several colours, arranged contiguous, or near to each other, as hereinbefore described; and which colouring apparatus I claim, whether the same be used in conjunction with the machinery before described, or with the common hand block, or otherwise, and under whatever other modification."

Fabrication of Gas for lighting from Soap-suds employed in cleansing Stuffs. By M. HOUZEAU MUIRON.*

A FEW years ago, the immense quantity of soap-suds employed in the city of Rheims, in preparing woollen stuffs was entirely lost. M. Houzeau Muiron conceived the idea of extracting from them the fatty matter, and of making an important application thereof. In fact, by submitting them to a regular purification, he has obtained a limpid oil, with which he succeeds in preparing the soaps in demand in commerce, while the residue of this purification serves for the advantageous production of a gas for lighting a part of the city.

The soap-suds collected in the shops, where they have become saturated with grease and the impurities of the tissues, are poured together into a large basin which is capable of containing about 3,000 gallons. To decompose them, there is poured upon them 308 pounds of muriatic acid, or 154 pounds of sulphuric acid, first diluted with its own weight of water, and the mass is rapidly agitated until the decomposition is complete.

Shortly afterwards a froth is seen to form, which at the end of twelve or eighteen hours is sufficiently well separated from the water

* "Jour. de Pharm. et de Chim.," May, 1842.

upon which it floats. Four-fifths of this water is then run off, containing about one per cent. of sulphate of potassa, which is utilized either by evaporating it in drying-houses, or by running it off upon dry earth exposed to the air, which, when sufficiently charged with the salt, is washed. Directly after this operation, the basin is filled again with a fresh portion of soap-suds, which float the fatty matter and permit it to be run off into a side tub. The product obtained is a mixture of unaltered oil, the acids, animal matters, and a large quantity of water which forms with them a species of hydrate. This water is disengaged by injecting several times into the mass a current of steam, which heats it and facilitates its evaporation. The fatty matter is then run off into a boiler, where it is submitted to a rapid ebullition, aided by continual agitation, which drives off the last portions of water. The product contains twenty or twenty-five per cent. of impure matters which colour it and render it turbid. To purify it, it is poured into basins of copper and mixed with two per cent. of concentrated sulphuric acid. After two days the limpid oil comes to the surface, while the impurities are precipitated to the bottom.

The oil is carefully separated, and the deposit, when filtered through cloths in a press, gives still a large quantity of oily products, which are added to the preceding and made into soap by treating them with common soda.

The residuum is black and very thick ; from it M. Houzeau produces the gas for lighting, but before introducing it into the retort, he liquifies it by means of the empyreumatic oil obtained in the preceding operation.

The gas thus prepared is purified by lime, and the water from the washing contains sufficient cyanide of calcium for the preparation of Prussian blue from it by treating it with sulphate of iron and washing the precipitate with muriatic acid.

This gas possesses a considerable lighting power, and in order to apply it to the lighting of the establishments scattered throughout the city of Rheims, M. Houzeau has contrived a manner of transporting it, at the same time simple, economical and free from danger.

On the Want of Identity between Microlite and Pyrochlore. By Charles Upham Shepard, M.D., Prof. Chem. in the Medical College of the State of South Carolina.*

Prof. Silliman having apprised me on my recent arrival in town, that a paper was about to appear in this journal by Messrs. Teschemacher and Hayes of Boston, the object of which would be to establish the identity of Microlite with Pyrochlore, and being permitted by the editors, I hasten to reply in the same number.

* From the "American Journal of Science and Arts."

The opinion of the identity originated with Mr. Teschemacher; and is expressed without the slightest reserve, resulting as he remarks, "from a close examination of several crystals of the mineral named microlite;" but Mr. T. singularly enough omits altogether the results of any researches bearing on the point to be established, if we except the mention of a mineralogical property which is simply descriptive, never characteristic, viz., the circumstance that in microlite, the colour is "transparent (!) straw-yellow to brick-red and dark brown!"* For the full confirmation of his opinion he then refers to the analysis of Mr. Hayes, which was undertaken at the request of the Chemical Society of Boston.

I shall first exhibit the mineralogical differences between the two minerals, in order to show the fallacy of any attempt to unite the two species on natural-history grounds; and shall then inquire what support the new view acquires from chemistry.

Pyrochlore.	*Microlite.*
Crystals, unmodified regular octahedrons.	Crystals, regular octahedron, with edges truncated and angles surmounted by four-sided pyramids, whose faces correspond to the octahedral planes.
Cleavage, none (fracture conchoidal).	Cleavage, octahedral, distinct.
Lustre, resinous to vitreous.	Lustre, resinous.
Colour, deep red-brown to black.	Colour, pale honey-yellow. Surfaces of crystals often reddish or blackish brown from implanted minerals.
Streak, brown.	Streak, pale yellowish white.
Translucent on the edges, to opaque.	Semi-transparent to translucent.
Hardness=4·75.	Hardness, 5·25 (readily scratches Pyrochlore).
Gravity=4·20 to 4·25.	Gravity=5·485 to 5·562.†

I consider it apparent therefore, that all attempts to unite two minerals, whose individuals afford no transition-links to constitute

* In this account of colour however, my specimens do not agree.

† The specific gravity as heretofore quoted, was 4·75 to 5·00; but very opportunely for this examination, I had brought with me from my Charleston cabinet an excellent crystal, weighing 3·805 grains; and possessing one of Robinson's best eight inch-beam balances (which turns readily with one-thousandth of a grain), I was able to correct the specific gravity, as above. The original determination of this property was effected by means of a balance much less sensitive, and on a crystal whose weight was only four-tenths of a grain. Indeed, this was the largest crystal I had seen, when I described the species. Observing however, that my crystal of 3·805 grains had two of its octahedral sides pitted from the implantation of calcareous spar and green tourmaline, I at first took its specific gravity with these impurities attached. The result was 5·485 I then cleaved off these faces, thereby reducing the crystal to 2·410 grains, when its gravity rose to 5·562.

the passage of insensible gradation, in properties so essential to specific gravity, internal structure and hardness, must prove unavailing; and according to the rules of forming the species in mineralogy, microlite must still stand distinct from pyrochlore.

Let us now turn to a review of the analysis of Mr. Hayes. This gentleman has not in any form, alluded to a paper of mine, entitled "Chemical Examination of Microlite," and published in vol. xxxii, p. 338 of this journal. To save the reader the trouble of referring to that article, I will simply quote from it so far as to say, that I found the microlite to be a columbate of lime and yttria, with moisture and traces of uranium and tungstic acid. The details of the examination were fully given in that communication; and in particular, the proofs that the mineral was a *columbate.* My surprise was therefore not inconsiderable, to find the subject of my former labours held up in a light so novel, as that of being a salt of a totally new and non-isomorphic genus.

The paper of Mr. Hayes in support of his views is out of proportion full on points where the evidence is unsatisfactory; and silent where the reader might appreciate its value as affecting the point at issue; for I hold the blowpipe characters of complex bodies like the microlite to be simply sufficient for furnishing the chemist to tolerable guesses, in aid of subsequent and more certain experiments with chemical reagents upon the decomposed and separated ingredients of the mineral. After nearly a page of blowpipe results, many, if not all of which would be likely in other hands (with different lamps, blowpipes, and lungs, as well as different sized specimens and proportions of fluxes), to be afforded from specimens of half a dozen other species as well as from microlite, we have the steps of the analysis given in the following words. "One grain of the mineral which had been dried was decomposed; the titanic acid carefully separated and dried, it weighed 80; this had the chemical characters of pure titanic acid.

"The solution of the oxide from the titanic acid gave a precipitate of sulphate of tin, when treated with sulphohydric acid, and the sulphate* oxidized before the blowpipe gave with soda a globule of tin. Sulphohydrate of ammonia gave a black precipitate, which when roasted contained oxides of iron and uranium, with traces of oxide of cerium. The fluid remaining gave with oxalate of ammonia, a precipitate which was converted into sulphate of lime, equivalent to 0·08 lime. Thus,

Titanic acid	80·0
Oxide of tin, iron, cerium, uranium	11·8
Lime	08·2—100·0

* The sulphuret of tin is here referred to by Mr. Hayes, in this as well as in the previous line.

"This mineral is therefore identical with that analyzed by Wöhler. The absence of protoxide of manganese and water, and the smaller proportion of oxide of iron in this specimen, as indicated by the blowpipe experiments, will account for the larger proportion of titanic acid given in the analysis."

Then follows a quotation of the analysis of pyrochlore by Wöhler, with which Mr. Hayes identifies his own results upon the microlite. But when we find, on comparing the two analyses, a difference of more than seventeen per cent. in the acid, and of one-third in the principal base, as well as a wide discrepancy in volatile matter, and call to mind that these results are deduced from a single trial, and that upon one grain's weight of the mineral, we must believe, that the conclusion of Mr. Hayes cannot be sustained upon such slender grounds; and, inasmuch as Mr. Teschemacher refers to the analysis for the confirmation of his views, is it not possible that Mr. H. in turn, has unconsciously been indebted to the mineralogical evidence for a portion of his confidence?

In order to speak from present knowledge on the subject, although I have no reason to doubt the correctness of former results, I subjected 2·142 grains of the above-mentioned crystal to the following examination; premising, however, that my inquiry was chiefly directed to the proofs of columbic acid in the mineral, and not to a rigid determination of proportions in the different constituents.

A. The powdered mineral was white, with only a faint tinge of yellow. Heated for half an hour to redness, it scarcely changed in colour; and lost 0·060 in weight, which equals 2·801 per cent. It was then intimately mixed with six times its weight of bisulphate of potash, and the mixture maintained in fusion for half an hour. Successive portions of water were boiled upon the fused mass for upwards of half an hour, until every thing soluble was taken into solution. A dense white powder remained undissolved.

B. Hydrosulphate of ammonia produced in a portion of the sulphate A, a precipitate, without sensitive discolouration, and which, from former proofs in my paper of 1835, was taken for yttria. The remainder of the fluid was then precipitated by oxalate of ammonia, the precipitate ignited, redissolved in hydrochloric acid, the yttria thrown down by ammonia, and the lime by oxalate of ammonia, which on ignition, weighed 0·032, or 1·49 per cent.*

C. The white insoluble matter A, was digested for some time in a saturated solution of hydrosulphate of ammonia, the operation being conducted on a filter in the way recommended by Berzelius. The insoluble matter was blackened by the affusion of the hydrosulphate.

* The calcareous content here obtained is quoted only as an approximation; but falling so much below what I previously found, and considered in conjunction with my observation of implanted calcareous spar upon the crystal, it is possible that lime may yet prove to be an accidental ingredient in the mineral.

D. The sulphohydric fluid was decomposed by nitric acid and heat; hydrochloric acid was added; no precipitation ensued. But the fluid, on the addition of ammonia, afforded a precipitate of peroxide of tin, which after ignition, weighed ·001, which equals 0·047 per cent. The oxide was reduced with carbonate of soda on charcoal to the metallic state.

E. The blackened powder C was treated with dilute hydrochloric acid, and gently warmed. The filtered solution was precipitated by ammonia, and ignited. The peroxide of iron was apparently blended with traces of yttria. It weighed 0·022, or about 1 per cent.

F. The insoluble matter, washed by hydrochloric acid (E), was drenched upon the filter with an abundance of hot water: it showed no tendency to pass through the filter in a milky state, as titanic acid is well known to do, under such circumstances. It was ignited in a platina crucible, and exhibited *a white colour while hot*, as well as after cooling. Its weight after some accidental losses was 1·052 gr., = 49·11 per cent. But I still regard my former determination of the proportion of acid, and which was 75·70 per cent., to be very near the truth.

A portion of the acid was fused with carbonate of potash in a platina crucible; water was boiled upon the mass, whereby *a complete solution was effected.* To a portion of the solution hydrochloric acid was added. It first occasioned a cloudiness, *but subsequently the fluid became clear. The same effect was produced by oxalic acid.* Ferrocyanide of potassium afforded with the oxalic solution *a yellow precipitate*, and the tincture of nut-galls with the same, a rich *orange precipitate.*

A portion of the acid was fused with biphosphate of soda on a platina hook, in the inner flame of the blowpipe: a transparent glass was obtained, which was *colourless while hot, as well as after cooling.* Fused with soda before the blowpipe, an opaque white bead was obtained.

My conclusion concerning the composition of the microlite, derived from the foregoing examination, coupled with that in 1837, is this, that it is a columbate of yttria and lime, with a little moisture, adventitious traces of iron, tin, tungstic acid, and uranium.

I do not possess enough of the mineral to enable me to undertake its analysis: but it is a question which I regard with interest, and shall welcome its final settlement from whatever quarter it comes; though I must be excused, in the face of such evidence as I have already cited, from acquiescing in the mere declaration of any authority however high, that its prime ingredient is titanic, in place of columbic acid.

To show that a zeal to defend a species because it was put forth by myself, is not the motive for the present reclamation, I am free to say, that I have long perceived several striking analogies which point towards an identity between microlite and the yellow yttrotan-

talite of Ytterby, although "a close examination" of several points in the history of the latter substance yet requires to be made, in order to establish the truth of such a conjecture.

New Haven, May 15, 1842.

On M. De Luc's *Electric Column.* *By* Thomas Forster, Esq.*

In consequence of M. De Luc's Electric Columns having been described in your "Philosophical Magazine," I think it right to inform your readers of a circumstance relating to it which may prove interesting. I have observed that the action of this column is materially influenced by the state of the atmosphere: your readers are acquainted with the manner in which two bells, attached to the plus and minus end of the column, are made to ring by means of an insulating conducting clapper being suspended between them, and with the circumstance of their having rung for many months together in an instrument of Mr. B. M. Forster's, at Walthamstow. Now I have observed that they sometimes pulsate very strong and regularly, at other times weak and regularly, at others strong and irregularly, or with intervals of quiescence, and sometimes both weak and irregularly; and these variations seem to me to be connected with peculiarities in the electric state of the atmosphere. For I cannot perceive that there is any correspondence between the kind of action of this column and the state of the hygrometer, barometer, or thermometer; but there seems to be a connection between it and certain appearances of the clouds, the peculiarities of which are (according to the modern theory) caused chiefly by electricity: when the air is dry, with strong easterly winds, when the cirrus cloud ramifying about in all directions, and occasionally accompanied by the other modifications, continues for a long time unattended by rain; when the nights are clear, and small meteors, called falling stars, are numerous; when I say these circumstances happen together, I have observed that the bells of this column always ring with very irregular pulsations; and further, when rain succeeds such kind of weather, it commonly happens that their pulsations become weak, or cease altogether, and the bells become silent: on the contrary, when the weather is fair, and when only diurnal cumuli prevail, they usually pulsate regularly. An ingenious meteorologist suggested to me the other day, that the irregular pulsation of the bells might be occasioned by the electric fluid's passing downward to the earth in pulsations, which might be the case when it was very irregularly distributed in the atmosphere.

To me it appears that this irregular distribution of the electric fluid would be indicated by the multiform appearance of the cirrus cloud which I have described, for the particular office of this cloud

* From "Tilloch's Philosophical Magazine."

seems to be that of serving as a conductor of electricity. The same circumstance would also give rise to the occasional appearance of other modifications. All however that can be said on this interesting subject at present is, that there seems to be a connection worth attending to between the kind of action of the column, and the kind of weather which prevails, indicated by the various and peculiar appearances of the clouds. Future observations may lead to the knowledge of adjunct circumstances which may have their share in producing these changes. To engage the co-operation of other meteorologists, by which alone the science can be brought to any degree of perfection, is the object I have in view in soliciting the favour that this may be inserted in your "Philosophical Magazine."

I remain, Sir,

Your constant reader and servant,

THOMAS FORSTER.

St. Helen's Place, June 14, 1811.

A Daguerreotype Experiment by Galvanic Light. *By* B. SILLIMAN, Jun., A.M., of the departments of Chemistry and Mineralogy in Yale College, and WM. HENRY GOODE, M.D.*

IN November, 1840, we succeeded in obtaining a photographic impression, by galvanic light reflected from the surface of a medallion to the iodized surface of a Daguerreotype plate. The large battery in the laboratory of Yale College, consisting of nine hundred pairs of plates, ten inches by four, was charged with a weak solution of sulphuric acid, and its poles adjusted with charcoal points, in the manner which is customary when an intense light is to be produced by means of this instrument. Two pictures were obtained, one of which is made up of a blur, or spot, produced by the light from the charcoal points, the image of the retort-stand, on which a medallion of white plaster rested, and the image of the medallion, but the lines on its face are not given. The camera was about six feet from the charcoal points when this impression was taken, and the medallion a little on one side, and in the rear of the points. The plate was exposed to the light about twenty seconds, and no means were employed either for condensing the light on the objects to be copied, or that reflected from them, on the lens which gave the image. The only lens employed was a French achromatic, three inches in diameter, and of about sixteen inches focal length. Another picture was taken of the medallion only, which was placed about two feet from the charcoal points, and the camera about four feet from it, and in such a position that the charcoal points did not come within the field of the lens. This picture, we regret to say, has been inadvertently destroyed. The plates used were of inferior quality, being some of the first of American manufacture.

* From the "American Journal of Science and Arts."

These experiments were not published at the time they were made, because it was understood, that a gentleman distinguished for his scientific investigations was already engaged in studying this branch of the subject, with whose researches we had no wish to interfere, and the matter was abandoned mainly for this reason. Having been informed recently, however, that this gentleman had also abandoned it, we have concluded to give this account of our own experiments.

On the same occasion, an observation was made respecting the image given by the two charcoal points, when they were nearly in contact, and the battery in full operation, which we do not remember to have met with elsewhere. An image of each charcoal point is given, separate from that of the other, by a lens placed at a little distance. These two images differ remarkably in colour; one is of the colour of the flame afforded by the combustion of an alcoholic solution of strontia; the other resembles in colour the flame produced by the combustion of an alcoholic solution of chloride of sodium, more nearly than any thing else with which we can compare it. The charcoal points were shifted, each to the opposite pole of the battery, without producing any change in the colour of the light given off by the poles respectively. Other pieces of charcoal were substituted, in the place of those with which this phenomenon was first observed, but the difference in the colour of the two images was always present, and did not seem to be connected in any manner with the particular charcoal points employed, but the yellow image was uniformly given by one pole, and the purple image by the other pole of the battery. We are under the impression that the yellow coloured image was produced from the charcoal point in connection with the positive pole of the battery, and that the strontia coloured image came from the negative pole of the battery, though of this no note was made at the time. No attempt was made to ascertain by direct experiments whether these images possessed a different degree of power or not, in producing an impression upon an iodized plate. The difference in their colour was presumptive evidence that one image (that from the negative pole), possessed more of the chemical rays than the other. But evidence is (we are of opinion) afforded indirectly that such is the fact. The light from both charcoal points made a slight impression on the iodized plate before they were brought so close together as to unite in forming a general blur: these two small spots or impressions are nearly opposite, or at each extremity of one diameter of the blur, and without its circumference; one of them is more distinct than the other. Within the edge of the blur, and nearly in the same diameter with the two spots above named, there are also two impressions, darker and more strongly marked than is the general impression made by the light from the points. One of these spots is doubtless made by the light from one point, while the other is due to the light from the other point, and one of them far exceeds the other in distinctness. Now the more strongly marked spot without the blur, and the more

strongly marked one within it, are close to each other on the same edge of the blur, and are doubtless produced by the light from one and the same charcoal point. The two other spots, viz., that without and that within the blur, which are much less distinct, are close to each other at the opposite extremity of the diameter of the blur, and are also evidently produced by the light from the other charcoal point.

Yale College Laboratory, June 20, 1842.

Description of a Method of fitting up in a portable form the Electric Column, lately invented by J. A. De Luc, Esq. *Also an Account of several Experiments made with it. By* B. M. Forster, Esq.*

Having been informed that a row of galvanic plates had been constructed without any fluid being interposed, and that it acted very sensibly on a gold-leaf electrometer, I formed one of about two hundred small circles of zinc, and the same number of blotting-paper and Dutch gold-leaf, the Dutch leaf being cemented on the paper with a solution of gum arabic; the blotting paper was double, two pieces were gummed or pasted together before the Dutch leaf was put on. Through these circles, or plates, a silken string was passed for connecting them together. This small instrument acted sufficiently powerfully on a very delicate gold-leaf electrometer to encourage me to make a row consisting of a greater number of plates. To the two hundred I added about three hundred more, using, instead of the Dutch metal, silver-leaf, and inserted the whole in a glass tube fitted up with brass caps, screws, and balls. The instrument thus fitted up may be called an *electric rod.* I have some of these rods with the plates not connected by a string through them; which, provided the glass tube is very nearly of the same diameter as the plates, may be the best way of placing them; but unless the tube fits accurately, the other mode will probably be found preferable, as the plates can be more easily placed regularly.

The Dutch metal, or silver-leaf, may either be fastened to the paper with gum, or paste made over the fire with flour and water.

The following experiments were made with a rod of five hundred series of plates; whether with the one in which were two hundred plates of Dutch metal, or in which there were none, but silver-leaf instead, it is not necessary to mention.

21st Sept., 1809. One leaf of an electrometer made of Dutch metal kept flapping to and from the side of the glass many times, when connected with the electric rod.

——— The ends of the rod being placed upon two electrometers, when the top of either of them was touched, the electrometer at the opposite end diverged more immediately.

* From Tilloch's "Philosophical Magazine."

22nd Sept. The rod was placed at the bottom of an electrometer; one leaf was attracted to the side and flapped several times. This experiment shows that the electric power of these piles or columns acts through a portion of air: I held the upper part of the electrometer in my hand during this experiment.

24th Sept. A small piece of Dutch metal was attracted up to the ball at the zinc pole of the rod, and adhered to it.

4th Oct. A very light ivory needle, turning on a point (like a magnetic needle), was attracted by the rod; when a finger or a key was placed near one end of the needle, and the ball at the end of the rod also near the same end on the opposite side, the needle vibrated backwards and forwards. The needle was insulated, I believe, by a piece of amber.

——— The needle, after having been touched by the silver-end pole, evidently receded from that pole; or, as it is commonly called, was repelled, having been charged with the same kind of electricity as that end of the rod possessed: the same effect was perceived when charged by the zinc pole.

——— One leaf of an electrometer (Dutch leaf) moved, when one of the balls on the rod was placed over the top, without being in contact with it.

15th Oct. The ivory needle vibrated between the balls of two rods, one of which was at the zinc pole, the other at the opposite pole.

One column which I have made, consists of about five hundred plates, each about ¼th of an inch in diameter. I have put at the zinc end a piece of cork cut like the head of a snake or eel, and at the other end another to resemble a tail. This column may be called an artificial electric eel *(Gymnotus electricus):* it is not inserted in a tube like the others, a silken string runs through the centre of the plates, which may be drawn tight; then wound round a pin which is in the mouth, or may be loosened if desirable. This eel acts powerfully on the electrometers. The power appears to me to vary much more than that of the columns in tubes: provided the outside of these tubes be dry, I do not know that the strength of their electric power changes.

18th Oct. Three rods, each of five hundred series, were supported upon insulated stands, and a plate of copper suspended at the silver pole of the combined apparatus; another plate was placed under this (as in the common electrical experiment of the dancing images), one very small piece (or more) of tissue-paper was attracted up and fell down, and a little image of the same paper reared up, and once remained suspended to the upper plate, but I could not make it dance up and down.

22nd Oct. One ball or both? of Cavallo's pocket electrometer diverged, when three rods were combined; the pith-balls are on wires. With these three rods I could not perceive the metallic taste in the mouth which is so perceptible even with a single piece of zinc and silver placed against the tongue. When the ball or balls? of the electrometer moved, the opposite end of the apparatus was touched.

A small pith-ball, suspended I believe with a *single* thread of a silk-worm, vibrated between two fixed pith-balls, one of which was connected with the apparatus, the other communicated with the table.

23rd Oct. A coated jar had a slight charge given to it with one of the electric rods. When the zinc pole charged the inside of the jar, that side gave signs of a *minus* state (as it is called), and the outside a *plus* state. This was shown by an electrometer, the leaves of which *diverged* when excited amber was holden near it, after it had received electricity from the inside, and *converged* when electrified from the outside of the bottle. From the usual effects of galvanism the contrary might have been expected; that is, that the zinc end of the column would have produced the *plus* state, and the silver end the *minus*.

24th Oct. With three rods combined, a small brass ball suspended by silk between two bells vibrated between them, causing them to ring: the bells were suspended from the ends of the apparatus. The next day, 25th Oct. (the jubilee day), having fixed on glass pillars two bells, and hung by silk a brass ball from the upper part of a piece of wire, I placed the bells in connexion with the ends of the combined apparatus, by means of bent wires laid on them: the apparatus and bells were left for near an hour, during which time the bells kept ringing, at times stopping for a short interval, then ringing again; the clapper sometimes was seen to rest against one of them, then appeared to be disengaged by a person moving in the room. Whether the disengagement was always owing to some slight shaking of the table, or whether it was sometimes in consequence of the ball having acquired electricity, and then being repelled, I am not quite clear. It appears not improbable but that the weight of the clapper may be so adapted to the power of the apparatus, as to cause small bells to continue ringing for several years without intermission; if so, we shall have a machine which, by those who do not consider the subject philosophically, will be called *a perpetual motion*. How long the column will continue to produce the electric fluid cannot at present (or perhaps ever) be determined. The principal difficulty to be overcome, in order to keep the clapper in continual motion throughout the different seasons of the year, appears to be the want of a very accurate *insulation* of the apparatus; for, if the glass tubes or pillars which support them are damp, the current of the electric fluid will not pass along in the proper direction for the experiment.

29th Nov. Five rods, each of five hundred series, were combined; with these, two small bells kept ringing on and off for more than four hours, part of which time I was not in the room, so cannot tell how often they might have stopped: the ringing sometimes began again evidently not from any shake, but I imagine from the clapper having become electrified, and then being (as it is usually called) repelled from the bell against which it rested. I placed three rods of five hundred series (insulated), in a box, and brought wires from the ends of this combined apparatus, which I made communicate

with two bells. I placed, on Tuesday, 27th February, 1810, this apparatus in a closet, where I left it until Sunday, 11th March, the bells continuing to ring (as far as my observations went), from the time they were put into the closet until that day; when they ceased. What was the cause of their stopping I do not know, but imagine it was owing to dampness. I cannot ascertain that they rang the whole time without stopping, but have no reason to believe otherwise. I intended some months ago to have sent you the description of the above-mentioned apparatus with experiments, but deferred sending it on account of Mr. De Luc's paper not being published, which he sent to the Royal Society, in March, last year, and which contains a description of the *electric column* and its properties. He hopes soon to publish it himself. In the mean time he has permitted me to communicate my account to you. I consider the invention of this column as the most important discovery in the science of electricity since that of the *voltaic pile,* and do not doubt but that when Mr. De Luc gives his paper to the public, it will prove extremely interesting, and I have reason to believe it may lead to further discoveries which will be considered as very important in this branch of science.

On Wednesday night, 14th March, I put into a closet a couple of bells, communicating with the three rods above mentioned in a box; they then began to ring, and are now ringing:—how long they will continue so I cannot say, perhaps some change in the weather may soon occasion the clapper to cease vibrating.

I remain, &c.,

B. M. Forster.

Walthamstow, Essex, 20th March, 1810.

On the Chemical Statics of Organized Beings. By M. Dumas.*

(*Continued from page* 293).

III. Let a seed be thrown into the earth, and be left to germinate and develope itself; let the new plant be watched until it has borne flowers and seeds in its turn, and we shall see, by suitable analyses, that the primitive seed, in producing the new being, has fixed carbon, hydrogen, oxygen, azote, and ashes.

Carbon.—The carbon originates essentially in carbonic acid, whether it be borrowed from the carbonic acid of the air, or proceed from that other portion of carbonic acid which the spontaneous decomposition of manures continually gives out in contact with the roots.

But it is from the air especially that plants most frequently derive their carbon. How could it be otherwise, when we see the enormous quantity of carbon which aged trees, for example, have appropriated to themselves, and yet the very limited space within which their

* "London and Edinburgh Philosophical Magazine."

roots can extend? Certainly, when a hundred years ago the acorn germinated which has produced the oak that we now admire, the soil on which it fell did not contain the millionth part of the carbon that the oak itself now contains. It is the carbonic acid of the air which has supplied the rest, that is to say, nearly the whole.

But what can be clearer and more conclusive than the experiment of M. Boussingault, in which peas, sown in sand, watered with distilled water, and having no aliment but air, have found in that air all the carbon necessary for development, flowering, and fructification?

All plants fix carbon, all borrow it from carbonic acid; whether this be taken directly from the air by the leaves, whether the roots imbibe within the ground the rain water impregnated with carbonic acid, or whether the manures, whilst decomposing in the soil, supply carbonic acid, which the roots also take possession of to transmit it to the leaves.

All these results may be proved without difficulty. M. Boussingault observed that vine leaves which were enclosed in a globe took all the carbonic acid from the air directed across the vessel, however rapid the current. M. Boucherie also observed enormous quantities of carbonic acid escape from the divided trunk of trees in full sap, evidently drawn by the roots from the soil.

But if the roots imbibe this carbonic acid within the earth, if this passes into the stalk and thence into the leaves, it ends by being exhaled into the atmosphere, without alteration, when no new force intervenes.

Such is the case with plants vegetating in the shade or at night. The carbonic acid of the earth filters through their tissues, and diffuses itself into the air. We say that plants produce carbonic acid during the night: we should say, in such a case, that plants transmit the carbonic acid borrowed from the soil.

But let this carbonic acid, proceeding from the soil, or taken from the atmosphere, come into contact with the leaves or the green parts, and let the solar light, moreover, intervene, then the scene all at once changes.

The carbonic acid disappears; bubbles of free oxygen arise on all the parts of the leaf, and the carbon fixes itself in the tissues of the plant.

It is a circumstance well worthy of interest, that these green parts of plants, the only ones which up to this time manifest this admirable phenomena of the decomposition of carbonic acid, are also endowed with another property not less peculiar, or less mysterious.

In fact, if their image were to be transferred into the apparatus of M. Daguerre, these green parts are not found to be reproduced there; as if all the chemical rays, essential to the Daguerrian phenomena, had disappeared in the leaf, absorbed and retained by it.

The chemical rays of light disappear, therefore, entirely in the green parts of plants; an extraordinary absorption, doubtless, but

which explains, without difficulty, the enormous expense of chemical force necessary for the decomposition of a body so stable as carbonic acid.

What, moreover, is the fixed function of this carbon in the plant? for what is it destined? For the greater part, without doubt, it combines with water or with its elements, thus giving birth to matters of the highest importance for the vegetable.

If twelve molecules of carbonic acid are decomposed and abandon their oxygen, the result will be twelve molecules of carbon; which, with ten molecules of water, may constitute either the cellular tissue of plants, or their ligneous tissue, or the starch and the dextrine which are produced from them.

Thus, in any plant whatever, nearly the entire mass of the structure (charpente), formed as it is of cellular tissue, of ligneous tissue, of starch, or of gummy matters, will be represented by twelve molecules of carbon united to ten molecules of water.

The ligneous part which is insoluble in water, the starch, which gelatinizes (l'amidon, qui fait empois) in boiling water, and the dextrine which dissolves so easily in water cold or hot, constitute, therefore, as M. Payen has so well proved, three bodies possessing exactly the same composition, but diversified by a different molecular arrangement.

Thus, with the same elements, in the same proportions, vegetable nature produces the insoluble walls of the cells of cellular tissue and of the vessels, or the starch which she accumulates as nourishment around buds and embryos, or the soluble dextrine which the sap can convey from one place to another for the wants of the plant.

How admirable is this fecundity, which out of the same body can make three different ones, and which allows of their being changed one into the other with the slightest expense of force every time occasion requires it!

It is also by means of carbon, united with water, that the saccharine matters so frequently deposited in the organs of plants for peculiar purposes, which we shall shortly mention, are produced. Twelve molecules of carbon and eleven molecules of water form the cane sugar. Twelve molecules of carbon and fifteen molecules of water make the sugar of the grape.

These ligneous, amylaceous, gummy, and saccharine matters, which carbon, taken in its nascent state, can produce by uniting with water, play so large a part in the life of plants, that, when they are taken into consideration, it is no longer difficult to understand the important part that the decomposition of carbonic acid performs in plants.

Hydrogen.—In the same manner that plants decompose carbonic acid for the appropriation of its carbon, and in order to form together with it all the neutral bodies which compose nearly their entire mass, in the same way, and for certain products which they form in less abundance, plants decompose water and fix its hydrogen. This

appears clearly from M. Boussingault's experiments on the vegetation of peas in closed vessels. It is still more evident from the production of fat or volatile oils so frequent in certain parts of plants, and always so rich in hydrogen. This can only come from water, for the plant receives no other hydrogenated product than the water itself.

These hydrogenated bodies, to which the fixation of the hydrogen borrowed from the water gives birth, are employed by plants for accessory uses. They form, indeed, the volatile oils which serve for defence against the ravages of insects; fat oils or fats, which surround the seed, and which serve to develope heat by oxidation (en se brûlent) at the moment of germination; waxes with which leaves and fruits are covered so as to become impermeable to water.

But all these uses constitute some accidents only in the life of plants; thus the hydrogenated products are much less necessary, much less common in the vegetable kingdom, than the neutral products formed of carbon and water.

Azote.—During its life every plant fixes azote, whether it borrows the azote from the atmosphere or takes it from the manure. In either case it is probable that the azote enters the plant, and acts its part there, only under the form of ammonia or of nitric acid.

M. Boussingault's experiments have proved that certain plants, such as Jerusalem artichokes, borrow a great quantity of azote from the air: that others, such as wheat, are, on the contrary, obliged to derive all theirs from manure: a valuable distinction for agriculture; for it is evident that all cultivation should begin by producing vegetables which assimilate azote from air, to rear by their aid the cattle which will furnish manure, and employ this latter for the cultivation of certain plants which can take azote from the manures only.

One of the most interesting problems of agriculture consists, then, in the art of procuring azote at a cheap rate. As for carbon, no trouble need be taken about it; nature has provided for it; the air and rain water suffice for it; but the azote of the air, that which the water dissolves and brings with it, the ammoniacal salts which rain water itself contains, are not always sufficient. With regard to most plants the cultivation of which is important, their roots should also be surrounded with azotated manure, a permanent source of ammonia or of nitric acid, which the plant appropriates as they are produced. This, as we know, is one of the great expenses of agriculture, one of its great obstacles, for it possesses only the manure which is of its own production. But chemistry is so far advanced in this respect, that the problem of the production of a purely chemical azotated manure cannot be long in being resolved.

M. Schattenman, the skilful director of the manufactories of Bouxvilliers in Alsace, M. Boussingault, and M. Liebig have turned their attention to the functions of ammonia in azotated manures. Recent trials show that the nitric acid of the nitrates also merit particular attention.

But for what purpose is this azote, of which plants seem to have such an imperious want? M. Payen's researches partly teach us; for they have proved that all the organs of the plant, without exception, begin by being formed of an azotated matter analogous to fibrine, with which at a later period are associated the cellular tissue, the ligneous tissue, and the amylaceous tissue itself. This azotated matter, the real origin of all the parts of the plant, is never destroyed; it is always to be found, however abundant may be the non-azotated matter which has been interposed between its particles.

This azote, fixed by plants, serves, therefore, to produce a concrete fibrinous substance, which constitutes the rudiment of all the organs of the vegetable.

It also serves to produce the liquid albumen which the coagulable juices of all plants contain; and the caseum, so often confounded with albumen, but so easy to recognise in many plants.

Fibrin, albumen, and caseum exist, then, in plants. These three products, identical in their composition, as M. Vögel has long since proved, offer a singular analogy with the ligneous matters, the amidon, and the dextrine.

Indeed, fibrin is, like ligneous matter, insoluble: albumen, like starch, coagulates by heat; caseum, like dextrine, is soluble.

These azotated matters, moreover, are neutral, as well as the three parallel non-azotated matters; and we shall see that by their abundance in the animal kingdom they act the same part that these latter exhibited to us in the vegetable kingdom.

Besides, in like manner as it suffices for the formation of non-azotated neutral matters, to unite carbon with water or with its elements, so, also, for the formation of these azotated neutral matters, it suffices to unite carbon and ammonium with the elements of water; forty-eight molecules of carbon, six of ammonium, and seventeen of water, constitute, or may constitute, fibrin, albumen, and caseum.

Thus, in both cases, reduced bodies, carbon or ammonium, and water, suffice for the formation of the matters which we are considering, and their production enters quite naturally into the circle of reactions, which vegetable nature seems especially adapted to produce.

The function of azote in plants is therefore worthy of the most serious attention, since it is this which serves to form the fibrin which is found as the rudiment in all the organs; since it is this which serves for the production of the albumen and caseum so largely diffused in so many plants, and which animals assimilate or modify according to the exigencies of their own nature.

It is in plants, then, that the true laboratory of organic chemistry resides. Thus carbon, hydrogen, ammonium, and water are the principles which plants elaborate: ligneous matter, starch, gums, and sugars on the one part, fibrin, albumen, caseum, and gluten, on the other, are then the fundamental products of the two kingdoms; products formed in plants, and in plants alone, and transferred by digestion into animals.

Ashes.—An immense quantity of water passes through the vegetable during the period of its existence. This water evaporates at the surface of the leaves, and necessarily leaves, as residue, in the plant the salts which it contained in solution. These salts compose the ashes, products evidently borrowed from the earth, to which, after their death, vegetables give it back again.

As to the form in which these mineral products deposit themselves in the vegetable tissue, nothing can be more variable. We may remark, however, that among the products of this nature, one of the most frequent and most abundant, is that pectinate of lime discovered by M. Jacquelain in the ligneous tissue of most plants.

IV. If, in the dark, plants act as simple filtres which water and gases pass through; if, under the influence of solar light they act as reducing apparatus which decompose water, carbonic acid, and oxide of ammonium, there are certain epochs and certain organs in which the plant assumes another, and altogether opposite, part.

Thus, if an embryo is to be made to germinate, a bud to be unfolded, a flower to be fecundated, the plant which absorbed the solar heat, which decomposed carbonic acid and water, all at once changes its course. It burns carbon and hydrogen; it produces heat; that is to say, it takes to itself the principal characters of animal life.

But here a remarkable circumstance reveals itself. If barley or wheat is made to germinate, much heat, carbonic acid and water are produced. The starch of these grains first changes into gum, then into sugar, then it disappears in producing carbonic acid, which the germ is to assimilate. Does a potato germinate? here, also, it is starch which changes into dextrine, then into sugar, and which at last produces carbonic acid and heat. Sugar, therefore, seems the agent by means of which plants develope heat as they need it.

How is it possible not to be struck from this with the coincidence of the following facts? Fecundation is always accompanied by heat. Flowers as they breathe produce carbonic acid: they therefore consume carbon; and if we ask whence this carbon comes, we see in the sugar cane, for example, that the sugar accumulated in the stalk has entirely disappeared when the flowering and fructification are accomplished. In the beet root, the sugar continues increasing in the roots until it flowers; the seed-bearing beet contains no trace of sugar in its root. In the parsnip, the turnip, and the carrot, the same phenomena take place.

Thus, at certain epochs, in certain organs, the plant turns into an animal; it becomes, like it, an apparatus of combustion; it burns carbon and hydrogen; it gives out heat.

But, at these same periods, it destroys in abundance the saccharine matters which it had slowly accumulated and stored up. Sugar, or starch turned into sugar, are then the primary substances by means of which plants develope heat as required for the accomplishment of some of their functions.

And if we remark with what instinct animals, and men too, choose

for their food just that part of the vegetable in which it has accumulated the sugar and starch which serve it to develope heat, is it not probable, that, in the animal economy, sugar and starch are also destined to act the same part, that is to say, to be burned for the purpose of developing the heat which accompanies the phenomenon of respiration?

To sum up, as long as the vegetable preserves its most habitual character, it draws from the sun heat, light, and chemical rays; from the air it receives carbon; from water it takes hydrogen; azote from the oxide of ammonium, and different salts from the earth. With these mineral or elementary substances, it composes the organised substances which accumulate in its tissues.

They are ternary substances, ligneous matter, starch, gums, and sugars.

They are quatenary substances, fibrin, albumen, caseum, and gluten.

So far, then, the vegetable is an unceasing producer; but if at times, if to satisfy certain wants, the vegetable becomes a consumer, it realises exactly the same phenomena which the animal will now set before us.

(To be continued).

Observations on Mr. DONOVAN'S *Reflections on the Inadequacy of the principal Hypotheses to account for the Phenomena of Electricity. By* G. A. DE LUC, Esq., F.R.S., &c.*

1. THIS paper of Mr. Donovan will, I hope, be very useful in settling the doctrine of electricity, against which he finds the objections detailed in his paper, upon its true foundation.

2. The doctrine of positive and negative electricities, first published by Franklin, is true in itself; but by the manner in which he had expressed it it was involved in many difficulties which he had not foreseen. I shall have occasion hereafter to relate the opportunity which I had, long after, to demonstrate to him personally, by an experiment, what kind of influence the air has in the propagation of the electric fluid.

3. Franklin's theory was first attacked by Dr. Peart, who pointed out some phenomena of electric motions absolutely inconsistent with his statement of his own system. I shall not enter into that discussion, but point out directly the source of an insurmountable difficulty in that theory, as it was first expressed by its author, which circumstance (as I shall successively prove) has been the only cause of its rejection by Mr. Donovan. The error was this: Dr. Franklin considered as the standard of plus and minus, or middle point between them, a certain natural quantity of electric matter belonging to all the bodies of the earth: he called negative the bo-

* From Tilloch's "Philosophical Magazine."

dies from which some part of the quantity was abstracted, and positive those to which a new quantity was added.

4. This error of Franklin has created all the just objections of Mr. Donovan; but from this circumstance I may judge that the latter has not had the opportunity of knowing two works which I have published, one in England, in 1787, in two volumes, under the title of "Idées sur la Météorologie:" the other was published at Paris, in 1804, also in two volumes, under the title of "Traité élémentaire sur le Fluide Electro-galvanique." In both these works I have applied Volta's system to the motions of a pair of balls, which motions are the most immediate test of electrical theories.

5. The essential and characteristic difference of Volta's theory, compared to that of Franklin, consists in the standard between plus and minus. Volta has demonstrated that there is no other standard or middle point between these two opposite electric states, than the actual electric state of the air, which possesses electric matter as well as the bodies which it embraces. But I must first relate the opportunity which I have had to learn that system from its author himself. Being at Paris in the year 1782, I made that very interesting acquaintance, and M. Volta was so good as to explain to me completely his system. The same year he came to London, and directed me in the construction of a more extensive set of electrical instruments, with which I made all the experiments related in the above mentioned works.

6. The principle of Volta with respect to electric motions, being that they have for their true standard the actual electric state of the ambient air, it came into my mind to submit that principle, as the foundation of the whole theory, to an *experimentum crucis*.

7. This experiment is related from p. 55 to 57 of the first volume of the "Traité élémentaire sur le Fluide Electro-galvanique:" where I first mention, as an indispensable condition for the success of that experiment, that the air be very dry; and I had the means, by my hygrometer, to determine the necessary degrees of dryness, which is about 43° of my scale. As these experiments require some time, a greater quantity of aqueous vapour mixed with the air, dissipates too fast the electric fluid accumulated upon bodies, and transmits too fast some electric fluid to those which have been rendered negative.

8. I made this series of experiments in two contiguous rooms, separated by a short passage and a door. In one of the rooms I placed a very strong electric machine, by which the air could be modified without affecting that of the other room. I had a pair of pith balls with long conducting threads, suspended to a brass cap at the extremity of a varnished glass rod, and thus thoroughly insulated, as well as the ferule to which the balls were suspended. Before the electric machine was put in motion in its room, the balls had no divergence in either of them.

9. In one of the experiments, I fixed a point at the extremity of the prime conductor of the machine, the rubber being placed in

communication with the ground: a luminous brush appeared at the point, indicating that the electric fluid escaped from it and was communicated to the air of the room. Now, when I brought the pair of balls from the next room, where they did not diverge, they strongly diverged in the room of the machine; and by the test of a rubbed stick of sealing-wax, they were found to diverge negatively, because the air had acquired some electric fluid by the action of the positive point, and thus was made positive; but when the balls were brought back to the other room, they ceased to diverge.

10. I made the inverse experiment, by fixing a point to the rubber of the machine, its prime conductor being placed in communication with the ground. The machine being worked in this state, a luminous point was seen at the extremity of the point fixed to the rubber, indicating that some electric fluid passed from the air of the room to the rubber, and thus that air was made negative. Now the pair of balls, which did not again diverge in the next room, being brought into that wherein the machine was, they strongly diverged, and by the test they diverged positively, being brought into an atmosphere rendered negative by the point fixed to the rubber.

11. These experiments cannot leave any doubt, on Volta's theory, that in the divergence of a pair of balls, the standard of plus and minus is not, as Franklin had determined it, a natural electric state of bodies; but that it is the actual and variable electric state of the ambient air. Had Mr. Donovan been acquainted with that theory of Volta, still retaining the fundamental theory of positive and negative, first announced by Franklin, but changing the standard from a fixed to a changeable state well defined, he would certainly have retained the fundamental doctrine of positive and negative with that correction.

12. As to the theory of Æpinus, that electric atmospheres do not exist, which Mr. Donovan seems to prefer to that of Franklin corrected by Volta, I have shown in one of my works, that Æpinus, instead of simplifying the theory, has fallen into a greater complication of hypotheses, even contrary to some general laws of nature. But I shall not enter into that discussion, as Volta's system renders it unnecessary.

13. It is by this influence of the air, receiving its share of the electric fluid possessed by the bodies which it surrounds, that Volta explained the motions of electrified balls, and the following is his explanation. "When two balls suspended near each other are in a positive state comparatively to the air of the place, they both communicate some electric fluid to the air between them; while each of them communicates alone the electric fluid to the outward air. Each of the balls therefore moves towards that outward air on each side, not by repelling each other, but by moving towards the air possessing less electric fluid."

14. During the course of my experiments to demonstrate, in various manners, the certainty of Volta's system on the cause of motion of electrified balls, it came into my mind that an analogous

phenomenon might be produced, in which the cause of the motion might be visible; and it having succeeded, I have described this experiment from p. 116 to 119 of the same volume, of which it will be sufficient to give here an extract.

15. An accidental observation led me to that experiment, which those who shave themselves and employ soap powder will easily repeat. If the basin used happens to be covered with dust, this swims over the water; but it is very easily swept, by scraping a small flake of soap, taking it on the point of a knife, and endeavouring to let it fall on the middle of the surface of the water with its convexity undermost. As soon as that flake of soap swims on the water, the dust recedes from it, and ascends to the edge of the water on the side of the basin, where it forms a thin fringe.

16. Having taken notice of that motion of the surface of water made visible by the dust, I attempted to imitate the motion of two electrified balls by two discs of soap half an inch in diameter, and about 1-10th of an inch thick, in the centre of which I fixed with gum a very thin thread four inches long, and I suspended these discs to a slip of wood at such a distance that they slightly touched each other. When that little apparatus was prepared, and the two discs made to rest at the same time on the surface of the water, they began to recede from each other, and the motion of the dust on the surface of the water visibly demonstrated that the discs of soap moved both towards the water which had not yet dissolved soap.

17. Lastly, I have demonstrated by a direct experiment, which I shall relate hereafter, that a perfect vacuum does not transmit the electric fluid; which fact will be a peremptory proof of the essential interference of air in the motion of a pair of electrified balls.

18. Another essential part of Volta's system, with which Mr. Donovan is not acquainted, and which when fully understood, removes all the difficulties he has found, is that which Volta has called electric influences. It consists in this effect: when a body positive is brought near one of the extremities of an insulated conductor, it gives more tension to the electric fluid at that end of the conductor, and makes it recede to the further end. This principle removes all the difficulties that Mr. Donovan has found, and in particular that which seems to arise from his experiment related in p. 338, of a pith ball suspended to a glass rod, and an excited glass tube brought under it. The excited tube occasions a slow retreat of electric matter along the thread. While that matter remains together in the ball, the thread, and at the point of suspension, the ball is repelled by the excited tube; but at last the suspended ball loses some electric matter, and being then negative, it is attracted by the excited tube.

19. The author attacks with reason the cause assigned by Franklin to the luminous brush which appears at the extremity of a pointed body fixed to the prime conductor of an electric machine in motion. But Volta has also explained this pheno-

menon in a satisfactory manner, as I have explained in pp. 60 and 61. Suppose a conductor positively electrified, and to which is presented another conductor in communication with the ground, but the surface of which is of a certain extent; the particles of air in motion, returning from the electrified conductor, and thus possessing more electric fluid, might transmit some to the other body; but their influence on it producing a diminution of tension in the fluid proceeding from the conductor, at the same time that they increase the tension in the fluid of the body, there is thus a very small disposition of the electric fluid to abandon the particles of air. But if it be a point, the electric fluid belonging to it is in too small a quantity to increase the tension of that belonging to the body: thus, every particle of air which comes in contact with that body discharges its electric fluid, returns instantly, and, as it takes the shorter way, thus is produced a current of air. The same alternate motions takes place in the particles of air, whether the point is negative or positive.

20. The difficulty concerning the impermeability of glass to the electric fluid, which the author opposes to the phenomena of the Leyden vial, as accounted for by Franklin, has also been removed by Volta, by supposing the electric fluid composed of two ingredients, to one of which only glass is impermeable, but the other passes readily through it. If the jar is coated on both sides with tinfoil up to a certain height, it will not receive a charge, unless the outside coating is in communication with the ground.

21. On the explanation of this common phenomenon rests the whole theory of positive and negative electricities, which Volta, having placed on its true foundation, has thereby clearly explained the phenomena of the Leyden vial; and it is his theory which has led me to my systems on the nature of the electric fluid. This fluid, considered in its common phenomena, is composed of two ingredients; one, which does not possess expansibility by itself; the other, which, united with it, gives expansibility to the compound. Of this union I have given an example in the aqueous vapour, which is composed of particles of water, a substance not possessing expansibility, and of particles of fire, which gives expansibility to the compound. That analogy, with all its particulars, I have explained in p. 77, sec. I, with this title: "Analogies and Differences of the Electric Fluid, and the Aqueous Vapour." In consequence of this analogy, I have given the name of electric matter to that ingredient of the electric fluid to which glass is not permeable, and deferent fluid to that ingredient which transports the electric matter, and which alone passes through the glass.

22. In this system Mr. Donovan may find the solution of his difficulties concerning the charge and discharge of the Leyden vial. Franklin's system is true so far as he supposes that the vial possesses (nearly) the same quantity of electric fluid, both when it is in its natural state, and when it is charged; but the electric fluid is not distributed in the same manner in both cases. When an electric

machine tends to produce an accumulation of electric fluid on one of the surfaces of the vial, it cannot accumulate except the outside of the vial can part with an equal quantity, its coating being in communication with the ground. But it is not the identical electric fluid which pervades the glass, the electric matter thrown in remains fixed on the inner surface; the deferent fluid alone pervades the glass, and, uniting with the electric matter on the outward surface, forms there an equal quantity of electric fluid, which is transmitted to the ground if it finds a conductor. This is a complete analogy with one of the phenomena of the aqueous vapour mentioned in the above quoted section. The maximum of charge is attained when the quantity of electric matter accumulated on the internal surface is in equilibrium with that transmitted by the electric machine.

23. So far this system might appear only an hypothesis to Mr. Donovan, as he does not know the work to which I refer, where I have given a peremptory demonstration of it, derived from the electric motions, proving by a direct experiment that these motions refer only to the electric matter, without interference of the deferent fluid, except to transport the electric matter.

24. In tome ii, p. 8, I have related an experiment which very much struck M. Cavallo, in presence of whom I made it at Windsor. The apparatus for that experiment is described, with a plate, in my work; it consists of a conductor supported in an horizontal position on an insulating pillar. At one end of this conductor are suspended, by brass wire, two cork-balls, half an inch diameter, going down so as to hang opposite to the centre of two insulated brass discs two inches diameter, brought to about 3¼ inches of each other, and the balls hung at equal distance from them. I give a spark to each of the discs with a Leyden vial: the deferent fluid of the discs, thus rendered positive, and giving more expansive power to the fluid of the balls, a part of it is carried towards the other extremity of the insulated conductor; and the balls losing thus a part of their electric matter, they diverge as being in a negative state, and are carried on both sides towards the positive discs, which they would strike, were they not prevented by a kind of bridle, placed at the end of the conductor, showing by small motions as it were an avidity to move towards them.

25. In that state of the apparatus in which the influence of the discs, being positive, gives more expansive power to the electric fluid of the balls, they are certainly negative comparatively to the ground and the ambient air; but the deferent fluid of the positive discs giving more expansive power to their electric matter, a conductor communicating with the ground may be brought into immediate contact with them without effect: their negative divergence still continues. The same phenomenon is produced by inversely electrifying the horizontal insulated conductor and the discs: when the balls, then positive, are made to communicate with the ground by a brass conductor, they continue to diverge, being under the influence of the negative atmosphere of the discs.

26. These phenomena much interested M. Cavallo, who was a very liberal-minded man. He persisted in his opinions as long as he found reasons to defend them, but was ready to abandon them whenever his arguments appeared to be contradicted by facts, to which he was very attentive. Having made some stay at Windsor, he came often to me and proposed some alterations in my apparatus, which he thought could change the phenomena. I foretold him what would happen by the changes, some of which would change the effect on account of causes which I pointed out, and others would produce the same effect. At last he saw and acknowledged that those phenomena could absolutely not be explained but by admitting the composition of the electric fluid of two ingredients, one of which only, which I had called electric matter, produced electric motions, and the other, which gave expansibility to that matter in proportion to its relative quantity, which I named electric deferent. Thus Volta's theory, applied to all the electric phenomena which were opposed to Franklin's theory, in his own expressions, have cleared the system of positive and negative electricities from the objections of Mr. Donovan, arising from the erroneous principle which he admitted in it, of a natural quantity of electric matter belonging to all terrestrial bodies.

27. I come to another very important object in the science of electricity, that of the effect of friction, which in vol. xxxiii, of Mr. Nicholson's "Philosophical Journal," I have explained in a manner different from that of Mr. Donovan in p. 337. His hypothesis is this: "That during attrition between bodies, one of which must be an electric, the pores of the latter being open will receive the plus quantity, and will give it out again when the pores close."—In this respect I have proved, that there is no other distinction between bodies than that of conductors and non-conductors; the latter of which only can be excited by friction, because the electric fluid, thus set in motion on the surface, moves slowly to make its escape by a conductor offered at a distance. While, if both bodies which exercise friction on each other are good conductors, the equilibrium is incessantly restored; but if one has more disposition than the other to attract the electric fluid thus agitated, with the faculty of transmitting it to its remote parts, when the bodies are separated before the equilibrium is restored between them, one is found positive and the other negative.

28. These effects are distinctly demonstrated by a small electric machine, the figure of which is at the head of my paper in Mr. Nicholson's "Philosophical Journal" for January, 1811, under the title of "Experiments concerning the Electric Machine, showing the Effects of Friction between Bodies."

29. M. Cavallo has given a table containing the results of his experiments of this kind, wherein we find that certain bodies become either positive or negative by friction, according to those by which they are rubbed; but the manner in which his experiments were made did not indicate the effect produced on the rubber itself,

because it was not insulated. I thought it therefore very important to the doctrine of electricity, to have both effects indicated by electrometers. This I obtained by the apparatus described in that paper; in which the bodies rubbed are spindles turned by a winch, and various rubbers made to press on the spindles by proper springs. A small insulated prime conductor is connected by one of its extremities to the spindles, and by the other with a gold-leaf electrometer. The rubbers are insulated, and each of them when applied is made to communicate with a similar gold-leaf electrometer.

30. The constant result of these experiments was, that the quantity of plus on one side, was equal to the quantity of minus on the other. Now the friction being reciprocal, the supposition of the opening of the pores cannot explain that phenomenon, since they ought to be opened equally in the rubbing as in the rubbed bodies.

31. The effect of friction, as I have said above, is to set in motion on the surface of the bodies, and that, if the body which recedes from the point of friction finds in its way a pointed conductor, it transmits to the latter a part of that fluid; which is the effect of the common electric machine: but the same phenomenon extends further; for that effect takes place between bodies of the same kind, if they be non-conductors. This is proved by Exper. 3, p. 9, of the same number of the "Philosophical Journal."

32. A flat piece of the same glass as the spindle, being held at the end of a brass spring, and used as a rubber on the spindle, holding the brass part in my hand near the glass, in order to restore from the ground the electric fluid carried away to the prime conductor by the spindle, the gold-leaf electrometer diverges as positive, though the rubber is of the same substance as the spindle. This is a peremptory demonstration that the effect of friction is not to open the pores for receiving more electric matter which is discharged when the friction ceases, as Mr. Donovan conceives it: since both bodies, which in this case exercise friction on each other, are of the same glass.

33. Experiment 4, p. 10, shows that sealing-wax, used as a rubber over the glass spindle becomes strongly negative, and renders the glass strongly positive. And thus it is directly proved, that the cause of sealing-wax being rendered negative by friction with the glass, is that the latter takes some electric fluid from sealing-wax, which effect could not have been ascertained without the two electrometers.

34. Now, in Exp. 1, a brass rubber applied to the glass cylinder is seen to become negative, and the glass cylinder made positive; but in Exp. 6, having covered a glass cylinder with a thick coating of sealing-wax, producing in fact a sealing-wax cylinder, the same brass rubber was rendered positive. These experiments compared with each other, further demonstrate the real electric effect of friction. But the next experiment will place it beyond all doubt.

35. Exp. 8, is made with what is called India beads, the size and colour of a cherry: they are made, as I have been informed, of an

inspissated vegetable oil, very elastic, not soluble in water. I placed one on a glass spindle, and fitted to its shape two narrow rubbers, one of naked brass, the other of brass covered with sealing-wax. With a very small motion of the winch, that bead became negative, and the naked brass rubber became positive; but the same bead became positive when the brass rubber was covered with sealing-wax, and this rubber was made negative.

36. These experiments afford a true analysis of the electric effects of friction. Its general effect, as I have stated, is to set in motion the electric fluid residing on the surface of the bodies which are rubbed; and the consequence of that effect is, that the body which recedes from the point of friction carries along with it a part of that electric fluid; which effect extends even to the case when the rubber and the body rubbed are of the same substance, when the latter suddenly recedes, and meets in its way a conductor, before returning to the rubber.

37. These experiments remove the difficulty which Mr. Donovan found (p. 339) to reconcile the (supposed) equal distribution of the electric fluid with the impermeability of glass. The equal distribution is gratuitously supposed, since glass is but an imperfect non-conductor when not covered with some resinous varnish.

38. Mr. Donovan says (p. 345): "As to the fact whether glass is actually impermeable, many experiments have been made; but they all appear to have been of doubtful force, and may be explained in some manner, without supposing that electricity passes through."

39. I have made in this respect some decisive experiments, proving that when a glass rod transmits the electric fluid it is only by its surface. I used for that purpose three glass rods of the same glass and the same diameter; one remaining naked, another covered all over with sealing-wax, the third covered with sealing-wax, with only an interruption in the middle of its length. These rods were supported horizontally on insulating pillars which left their extremities accessible to the knob of a Leyden vial. I used three very sensible electrometers, each having a long brass conductor in order to connect them with the rods.

Exp. 1. Having placed the naked rod on the pillars, with an electrometer at each end and one in the middle, and applied the Leyden vial to one extremity, the electrometer near it first diverged, then that in the middle, and soon that of the other extremity, showing that some time was required for the propagation of the electric fluid, even on the naked surface of glass.

Exp. 2. Having used the rod entirely covered with sealing-wax, a small motion was produced in the next electrometer, by the influence of the vial, but none in the remote electrometer.

Exp. 3. The rod with an interruption of the sealing-wax in the middle of its length was to be the test. If the glass were permeable the electrometer applied to that part ought to have been put in motion; but if it moves only along the surface of the glass, being stopped by the sealing-wax, the electrometer applied to the naked

part in the middle cannot be put in motion. The result of the experiment was, that whatever time the Leyden vial remained applied to that extremity, no motion was produced in that electrometer any more than in that of the other extremity. It is therefore demonstrated that glass is absolutely impermeable to the electric fluid.

40. A very specious objection of Mr. Donovan against Franklin's doctrine is thus expressed, p. 349: "Franklin supposes that no electricity can be received on one surface, unless the other can part with an equal quantity. In the case of excitation of the common cylinder, the inner surface having no communication with conductors, can part with none. How then can its outer surface receive the the great quantity we find in it?" The answer is however obvious, for this is the common effect of friction. The cylinder receives constantly electric fluid from the rubber, especially when this is covered with a metallic amalgama: for it is in communication with the ground. In that case it is not necessary that the opposite surface of the glass should part with any electric fluid; the whole process takes place on the outward surface. The rubber gives electric fluid to the glass cylinder, which parts constantly with it in meeting the prime conductor; but the rubber communicating with the ground, furnishes also constantly a new quantity of the fluid.

41. It remains only to state a very essential point in electric phenomena, namely, How does the electric fluid communicate itself through space? This was the object of an experiment which I made in 1774, in Mr. Walsh's laboratory, which experiment in the first volume of my work, "Idées sur la Météorologie," p. 521, I left under Mr. Walsh's name, because he had published it, without my knowledge, in the "Philosophical Transactions," for 1785. But these experiments were concerted between Dr. Franklin and myself, and only in the house and in presence of Mr. Walsh, with an apparatus of which the description will show the purpose and its origin.

42. In my experiments for producing a truly comparable barometer, repeating those of a French academician, Du Fay, for producing light in the vacuum of barometers, an experiment related in the "Mém. de l'Académie des Sciences de Paris," for 1723, I found, as has been explained in my work, "Recherches sur les Modifications de l'Atmosphère," tom. i, p. 43, that when the Torricellian vacuum was procured by making the mercury boil in the tube, no light appeared at the top of the barometer; whence I concluded that a perfect vacuum was not a conductor of the electric fluid.

43. Having expressed this idea to Dr. Franklin, he proposed to me an experiment, very difficult to execute, but which he encouraged me to undertake. The apparatus consisted of a glass syphon, the legs of which were about three feet distant from each other; the curve began about three inches above the point at which stood the common barometer, at that moment. When the syphon was filled with mercury it was inverted, with its two legs plunged into separate cups, each resting on an insulating stand: these cups received the mercury descending from the upper curve, and the

column thus separating, there were two barometers with a common vacuum.

44. In that situation, when a spark was given with a Leyden vial to one of the cups, a luminous arch was seen filling the top of the syphon, and a spark could be drawn from the other cup, thus showing that it was ~~not~~ a conductor of the electric fluid.

45. There remained to be performed the second part of the experiment and the most difficult, namely, to have the mercury boil in that long syphon. I succeeded in this operation; and when the syphon was placed with its legs in the cups, that complete vacuum ceased to be a conductor. The rain sparks were given to one cup, none was drawn from the other.

46. This experiment convinced Dr. Franklin of my system, that the electric fluid was a sort of parasite substance, which, on our globe and its atmosphere, was distributed on all other matters, and nowhere accumulated so as to produce lightnings, and their common attendants thunders; but that these phenomena proceed from the decomposition of the atmospheric air by a certain process, which manifests that the electric fluid enters into the composition of that air, a fluid *sui generis,* and not a mixture of different kinds of air, as it was supposed in the new chemical theory. Which conclusion of my long course of experiments serves as the basis of my work under the title of "Introduction à la Physique terrestre par les Fluides Expansibles."

Opinions and Conjectures concerning the Properties and Effects of the Electrical Matter, arising from Experiments and Observations made at Philadelphia, 1794.*

1. The electrical matter consists of particles extremely subtle, since it can permeate common matter, even the densest metals, with such ease and freedom as not to receive any perceptible resistance.

2. If any one should doubt whether the electrical matter passes through the substance of bodies, or only over and along their surfaces, a shock from an electrified large glass jar, taken through his own body, will probably convince him.

3. Electrical matter differs from common matter in this, that the parts of the latter mutually attract, those of the former mutually repel each other. Hence the appearing divergency in a stream of electrified effluvia.

4. But though the particles of electrical matter do repel each other, they are strongly attracted by all other matter.†

5. From these three things, the extreme subtilty of the electrical matter, the mutual repulsion of its parts, and the strong attraction

* From "Franklin's Experiments in Electricity."

† See the ingenious "Essays on Electricity," in the "Transactions," by Mr. Ellicot.

between them and other matter, arise this effect, that when a quantity of electrical matter is applied to a mass of common matter, of any bigness or length, within our observation (which hath not already got its quantity), it is immediately and equally diffused through the whole body.

6. Thus common matter is a kind of spunge to the electrical fluid. And as a spunge would receive no water if the parts of water were not smaller than the pores of a spunge; and even then but slowly, if there were not a mutual attraction between those parts and the parts of the spunge; and would imbibe it faster, if the mutual attraction among the parts of the water did not impede, some force being required to separate them; and fastest, if, instead of attraction, there were a mutual repulsion among those parts, which would act in conjunction with the attraction of the spunge. So is the case between the electrical and common matter.

7. But in common matter there is (generally) as much of the electrical as it will contain within its substance. If more is added it lies without upon the surface, and forms what we call an electrical atmosphere; and then the body is said to be electrified.

8. 'Tis supposed that all kinds of common matter do not attract and retain the electrical matter with equal strength and force, for reasons to be given hereafter. And that those called electrics *per se*, as glass, &c., attract and retain it strongest, and contain the greatest quantity.

9. We know that the electrical fluid is in common matter, because we can pump it out by the globe or tube. We know that common matter has near as much as it can contain, because, when we add a little more to any portion of it, the additional quantity does not enter, but forms an electrical atmosphere. And we know that common matter has not (generally) more than it can contain, otherwise all loose portions of it would repel each other, as they constantly do when they have electric atmospheres.

10. The beneficial uses of this electric fluid in the creation we are not yet well acquainted with, though doubtless such there are, and those very considerable; but we may see some pernicious consequences that would attend a much greater proportion of it. For had this globe we live on as much of it in proportion as we can give to a globe of iron, wood, or the like, the particles and other light matters that can get loose from it, would, by virtue of their separate electrical atmospheres, not only repel each other, but be repelled from the earth, and not easily be brought to unite to it again; whence our air would continually be more and more clogged with foreign matter, and grow unfit for respiration. This affords another occasion of adoring that wisdom which has made all things by weight and measure!

11. If a piece of common matter be supposed entirely free from electrical matter, and a single particle of the latter be brought nigh, it will be attracted, and enter the body, and take place in the centre,

or where the attraction is in every way equal. If more particles enter, they take their places where the balance is equal between the attraction of the common matter and their own mutual repulsion. 'Tis supposed they form triangles, whose sides shorten as their number increases, till the common matter has drawn in so many, that its whole power of compressing those triangles by attraction, is equal to their whole power of expanding themselves by repulsion, and then will such piece of matter receive no more,

12. When part of this natural proportion of electrical fluid is taken out of a piece of common matter, the triangles formed by the remainder are supposed to widen by the mutual repulsion of the parts, until they occupy the whole piece.

13. When the quantity of electrical fluid, taken from a piece of common matter, is restored again, it enters; the expanded triangles being again compressed till there is room for the whole.

14. To explain this: take two apples, or two balls of wood or other matter, each having its own natural quantity of the electrical fluid. Suspend them by silk lines from the ceiling. Apply the wire of a well charged vial held in your hand to one of them, and it will receive from the wire a quantity of the electrical fluid; but will not imbibe it, being already full. The fluid therefore will flow round its surface, and form an electrical atmosphere. Bring A into contact with B, and half the electrical fluid is communicated, so that each has now an electrical atmosphere, and therefore they repel each other. Take away these atmospheres, by touching the balls, and leave them in their natural state: then, having fixed a stick of sealing-wax to the middle of the vial to hold it by, apply the wire to A, at the same time the coating touches B. Thus will a quantity of the electrical fluid be drawn out of B, and thrown on A. So that A will have a redundance of this fluid, which forms an atmosphere round it, and B an exactly equal deficiency. Now, bring these balls again into contact, and the electrical atmosphere will not be divided between A and B into two smaller atmospheres as before, for B will drink up the whole atmosphere of A, and both will be found again in their natural state.

15. The form of the electrical atmosphere is that of the body it surrounds. This shape may be rendered visible in a still air, by raising a smoke from dry rosin dropt into a hot tea-spoon under the electrised body, which will be attracted, and spread itself equally on all sides, covering and concealing the body. And this form it takes because it is attracted by all parts of the surface of the body, though it cannot enter the substance already replete. Without this attraction, it would not remain round the body, but dissipate in the air.

16. The atmosphere of electrical particles surrounding an electrified sphere, is not more disposed to leave it, or more easily drawn off from any one part of the sphere than another, because it is equally attracted by every part. But that is not the case with bodies of any other figure. From a cube it is more easily drawn at the corners

than at the plane sides, and so from the angles of a body of any other form, and still most easily from the angle that is most acute. Thus if a body shaped as A, B, C, D, E, in Fig. 2, Plate 2, be electrified, or have an electrical atmosphere communicated to it, and we consider every side as a base on which the particles rest, and by which they are attracted, one may see, by imagining a line from A to F, and another from E to G, that the portion of the atmosphere included in F, A, E, G, has the line A, E, for its basis. So the portion of atmosphere included in H, A, B, I, has the line A, B, for its basis. And likewise the portion included, in K, B, C, L, has B, C, to rest on; and so on the other side of the figure. Now if you would draw off this atmosphere with any blunt smooth body, and approach the middle of the side A, B, you must come very near, before the force of your attractor exceeds the force or power with which that side holds its atmosphere. But there is a small portion between I, B, K, that has less of the surface to rest on, and to be attracted by, than the neighbouring portions, while at the same time there is a mutual repulsion between its particles, and the particles of those portions, therefore here you can get it with more ease, or at a greater distance. Between F, A, H, there is a larger portion that has yet a less surface to rest on, and to attract it; here therefore you can get it away still more easily. But easiest of all between L, C, M, where the quantity is largest, and the surface to attract and keep it back the least. When you have drawn away one of these angular portions of the fluid, another succeeds in its place, from the nature of fluidity, and the mutual repulsion before mentioned; and so the atmosphere continues flowing off at such angle, like a stream, till no more is remaining. The extremities of the portions of atmosphere over these angular parts, are likewise at a greater distance from the electrified body, as may be seen by the inspection of the above figure; the point of the atmosphere of the angle C, being much farther from C, than any other part of the atmosphere over the lines C, B, or B, A: and, besides the distance arising from the nature of the figure, where the attraction is less, the particles will naturally expand to a greater distance by their mutual repulsion. On these accounts we suppose electrified bodies discharge their atmosphere upon unelectrified bodies more easily, and at a greater distance from their angles and points than from their smooth sides. Those points will also discharge into the air when the body has too great an electrical atmosphere, without bringing any non-electric near to receive what is thrown off: for the air, though an electric *per se*, yet has always more or less water and other non-electric matters mixed with it: and these attract and receive what is so discharged.

17. But points have a property by which they draw on as well as throw off the electrical fluid at greater distances than blunt bodies can. That is, as the pointed part of an electrified body will discharge the atmosphere of that body, or communicate it farthest to another body, so the point of an unelectrified body will draw

off the electrical atmosphere from an electrified body, farther than a blunter part off the same unelectrified body will do. Thus a pin held by the head, and the point presented to an electrified body, will draw off its atmosphere at a foot distance; where, if the head were presented instead of the point, no such effect would follow. To understand this, we may consider that if a person standing on the floor would draw off the electrical atmosphere from an electrified body, an iron crow and a blunt knitting-needle held alternately in his hand, and presented for that purpose, do not draw with different forces in proportion to their different masses. For the man, and what he holds in his hand, be it large or small, are connected with the common mass of unelectrified matter; and the force with which he draws is the same in both cases, it consisting in the different proportion of electricity in the electrified body and that common mass. But the force with which the electrified body retains its atmosphere by attracting it, is proportioned to the surface over which the particles are placed; *i.e.*, four square inches of that surface retain their atmosphere with four times the force that one square inch retains its atmosphere. And as in plucking the hairs from the horse's tail, a degree of strength not sufficient to pull away a handful at once, could yet easily strip it hair by hair; so a blunt body presented cannot draw off a number of particles at once, but a pointed one, with no greater force, takes them away easily, particle by particle.

18. These explanations of the power and operation of points when they first occurred to me, and while they first floated in my mind, appeared perfectly satisfactory; but now I have written them, and considered them more closely, I must own I have some doubts about them; yet, as I have at present nothing better to offer in their stead, I do not cross them out: for even a bad solution read, and its faults discovered, has often given rise to a good one, in the mind of an ingenious reader.

19. Nor is it of much importance to us to know the manner in which nature executes her laws; it is enough if we know the laws themselves. It is of real use to know that china left in the air unsupported will fall and break; but how it comes to fall, and why it breaks, are matters of speculation. It is a pleasure indeed to know them, but we can preserve our china without it.

20. Thus in the present case, to know this power of points may possibly be of some use to mankind, though we should never be able to explain it. The following experiments, as well as those in my first paper, show this power. I have a large prime conductor, made of several thin sheets of clothier's pasteboard, formed into a tube, near ten feet long and a foot diameter. It is covered with Dutch embossed paper, almost totally gilt. This large metallic surface supports a much greater electrical atmosphere than a rod of iron of fifty times the weight would do. It is suspended by silk lines, and when charged will strike at near two inches distance a pretty hard stroke, so as to

make one's knuckle ache. Let a person standing on the floor present the point of a needle at twelve or more inches distance from it, and while the needle is so presented, the conductor cannot be charged, the point drawing off the fire as fast as it is thrown on by the electrical globe. Let it be charged, and then present the point at the same distance, and it will suddenly be discharged. In the dark you may see the light on the point when the experiment is made; and if the person holding the point stands upon wax, he will be electrified by receiving the fire at that distance. Attempt to draw off the electricity with a blunt body, as a bolt of iron round at the end and smooth (a silversmith's iron punch, inch thick, is what I use), and you must bring it within the distance of three inches before you can do it, and then it is done with a stroke and crack. As the pasteboard tube hangs loose on silk lines, when you approach it with the punch iron, it likewise will move towards the punch, being attracted while it is charged; but if, at the same instant, a point be presented as before, it retires again, for the point discharges it. Take a pair of large brass scales, of two or more feet beam, the cords of the scales being silk. Suspend the beam by a pack-thread from the ceiling, so that the bottom of the scales may be about a foot from the floor: the scales will move round in a circle by the untwisting of the pack-thread. Set the iron punch on the end upon the floor, in such a place as that the scales may pass over it in making their circle: then electrify one scale, by applying the wire of a charged vial to it. As they move round, you see that scale draw nigher to the floor, and dip more when it comes over the punch; and if that be placed at a proper distance, the scale will snap and discharge its fire into it. But if a needle be stuck on the end of the punch, its point upwards, the scale, instead of drawing nigh to the punch, and snapping, discharges its fire silently through the point, and rises higher from the punch. Nay, even if the needle be placed upon the floor near the punch, its point upwards, the end of the punch, though so much higher than the needle, will not attract the scale and receive its fire, for the needle will get it and convey it away, before it comes nigh enough for the punch to act. And this is constantly observable in these experiments, that the greater quantity of electricity on the pasteboard tube, the farther it strikes or discharges its fire, and the point likewise will draw it off at a still greater distance.

Now if the fire of electricity and that of lightning be the same, as I have endeavoured to show at large in a former paper, this pasteboard tube and these scales may respresent electrified clouds. If a tube of only ten feet long will strike and discharge its fire on the punch at two or three inches distance, an electrified cloud of perhaps 10,000 acres may strike and discharge on the earth at a proportionally greater distance. The horizontal motion of the scales over the floor, may represent the motion of the clouds over the earth; and the erect iron punch, a hill or high building; and then

we see how electrified clouds passing over hills or high buildings at too great a height to strike, and may be attracted lower till within their striking distance. And lastly, if a needle fixed on the punch with its point upright, or even on the floor below the punch, will draw the fire from the scale silently at a much greater than the striking distance, and so prevent its descending towards the punch; or if in its course it would have come nigh enough to strike, yet being first deprived of its fire it cannot, and the punch is thereby secured from the stroke. I say, if these things are so, may not the knowledge of this power of points be of use to mankind, in preserving houses, churches, ships, &c., from the stroke of lightning, by directing us to fix on the highest parts of those edifices, upright rods of iron made sharp as a needle, and gilt to prevent rusting, and from the foot of those rods a wire down the outside of the building into the ground, or down round one of the shrouds of a ship, and down her side till it reaches the water? Would not these pointed rods probably draw the electrical fire silently out of a cloud before it came nigh enough to strike, and thereby secure us from that most sudden and terrible mischief.

21. To determine the question whether the clouds that contain lightning are electrified or not, I would propose an experiment to be tried where it may be done conveniently. On the top of some high tower or steeple, place a kind of sentry-box, big enough to contain a man and an electrical stand. From the middle of the stand let an iron rod rise and pass bending out of the door, and then upright twenty or thirty feet, pointed very sharp at the end. If the electrical stand be kept clean and dry, a man standing on it when such clouds are passing low might be electrified and afford sparks, the rod drawing fire to him from a cloud. If any danger to the man should be apprehended (though I think there would be none) let him stand on the floor of his box, and now and then bring near to the rod the loop of a wire that has one end fastened to the leads, he holding it by a wax handle; so the sparks, if the rod is electrified, will strike from the rod to the wire, and not affect him. See Fig. 3, plate II.

22. Before I leave this subject of lightning, I may mention some other similarities between the effects of that, and those of electricity. Lightning has often been known to strike people blind. A pigeon that we struck dead to appearance by the electrical shock, recovering life, drooped about the yard several days, eat nothing, though crumbs were thrown to it, but declined and died. We did not think of its being deprived of sight; but afterwards a pullet struck dead in like manner, being recovered by repeatedly blowing into its lungs, when set down on the floor ran headlong against the wall, and on examination appeared perfectly blind. Hence we concluded that the pigeon also had been absolutely blinded by the shock. The biggest animal we have yet killed, or tried to kill with the electrical stroke, was a well-grown pullet.

23. Reading in the ingenious Dr. Miles's account of the thunder

storm at Stretham, the effect of the lightning in the stripping off all the paint that had covered a gilt moulding of a pannel of wainscot, without hurting the rest of the paint, I had a mind to lay a coat of paint over the filletting of gold on the cover of a book, and try the effect of a strong electrical flash sent through that gold from a charged sheet of glass. But having no paint at hand, I pasted a narrow strip of paper over it; and when dry, sent the flash through the gilding, by which the paper was torn off from end to end, with such force, that it was broke in several places, and in others brought away part of the grain of the Turkey-leather in which it was bound; and convinced me, that had it been painted, the paint would have been stript off in the same manner with that on the wainscot at Stretham.

24. Lightning melts metals, and I hinted in my paper on that subject, that I suspected it to be a cold fusion; I do not mean a fusion by force of cold, but a fusion without heat. We have also melted gold, silver, and copper, in small quantities by the electrical flash. The manner is this: Take a gold leaf, leaf silver, or leaf gilt copper, commonly called leaf brass, or Dutch gold; cut off from the leaf long narrow strips, the breadth of a straw. Place one of these strips between two strips of smooth glass that are about the width of your finger. If one strip of gold, the length of the leaf, be not long enough for the glass, add another to the end of it, so that you may have a little part hanging out loose at each end of the glass. Bind the pieces of glass together from end to end with strong silk thread; then place it so as to be part of an electrical circuit (the ends of gold hanging out being of use to join with the other parts of the circuit), and send the flash through it, from a large electrified jar or sheet of glass. Then if your strips of glass remain whole, you will see that the gold is missing in several places, and instead of it a metallic stain on both the glasses; the stains on the upper and under glass exactly similar in the minutest stroke, as may be seen by holding them to the light; the metal appeared to have been not only melted, but even vitrified, or otherwise so driven into the pores of the glass as to be protected by it from the action of the strongest *aqua fortis* or *aqua regia*. I send you enclosed two little pieces of glass with these metallic stains upon them, which cannot be removed without taking part of the glass with them. Sometimes the stain spreads a little wider than the breadth of the leaf, and looks brighter at the edge, as by inspecting closely you may observe in these. Sometimes the glass breaks to pieces; once the upper glass broke into a thousand pieces, looking like coarse salt. These pieces I send you were stained with Dutch gold. True gold makes a darker stain, somewhat reddish; silver a greenish stain. We once took two pieces of thick looking-glass, as broad as a Gunter's scale, and six inches long; and placing leaf gold between them, put them between two smoothly-planed pieces of wood, and fixed them tight in a bookbinder's small press; yet though they were so closely confined, the force of the electrical shock

shivered the glass into many pieces. The gold was melted, and stained into the glass, as usual. The circumstances of the breaking of the glass differ much in making the experiment, and sometimes it does not break at all: but this is constant, that the stains in the upper and under pieces are exact counterparts of each other. And though I have taken up the pieces of glass between my fingers immediately after this melting, I never could perceive the least warmth in them.

25. In one of my former papers, I mentioned, that gilding on a book, though at first it communicated the shock perfectly well, yet failed after a few experiments, which we could not account for. We have since found that one strong shock breaks the continuity of the gold in the filletting, and makes it look rather like dust of gold, abundance of its parts being broken and driven off; and it will seldom conduct above one strong shock. Perhaps this may be the reason: When there is not a perfect continuity in the circuit, the fire must leap over the vacancies: There is a certain distance which it is able to leap over according to its strength; if a number of small vacancies, though each be very minute, taken together exceed that distance, it cannot leap over them, and so the shock is prevented.

26. From the before-mentioned law of electricity, that points as they are more or less acute, draw on and throw off the electrical fluid with more or less power, and at greater or less distances, and in larger or smaller quantities in the same time, we may see how to account for the situation of the leaf of gold suspended between two plates, the upper one continually electrified, the under one in a person's hand standing on the floor. When the upper plate is electrified, the leaf is attracted, and raised towards it, and would fly to that plate were it not for its own points. The corner that happens to be uppermost when the leaf is rising, being a sharp point, from the extreme thinness of the gold, draws and receives at a distance a sufficient quantity of the electric fluid to give itself an electric atmosphere, by which its progress to the upper plate is stopt, and it begins to be repelled from that plate, and would be driven back to the under plate, but that its lowest corner is likewise a point, and throws off or discharges the overplus of the leaf's atmosphere, as fast as the upper corner draws it on. Were these two points perfectly equal in acuteness, the leaf would take place exactly in the middle space, for its weight is a trifle compared to the power acting on it: but it is generally nearest the unelectrified plate, because, when the leaf is offered to the electrified plate, at a distance, the sharpest point is commonly first affected and raised towards it; so that point, from its greater acuteness, receiving the fluid faster than its opposite can discharge it at equal distances, it retires from the electrified plate, and draws nearer to the unelectrified plate, till it comes to a distance where the discharge can be exactly equal to the receipt, the latter being lessened, and the former increased; and

there it remains as long as the globe continues to supply fresh electrical matter. This will appear plain, when the difference of acuteness in the corners is made very great. Cut a piece of Dutch gold, (which is fittest for these experiments on account of its greater strength) into the form of Fig. 4, the upper corner a right angle, the two next obtuse angles, and the lowest a very acute one; and bring this on your plate under the electrified plate, in such a manner as that the right-angled part may be first raised (which is done by covering the acute part with the hollow of your hand) and you will see this leaf take place much nearer to the upper than the under plate; because without being nearer, it cannot receive so fast at its right-angled point as it can discharge at its acute one. Turn this leaf with the acute part uppermost, and then it takes place nearest the unelectrified plate; because, otherwise, it receives faster at its acute point than it can discharge at its right-angled one. Thus the difference of distance is always proportioned to the difference of acuteness. Take care in cutting your leaf, to leave no little ragged particles on the edges, which sometimes form points where you would not have them. You may make this figure so acute below, and blunt above, as to need no under plate, it discharging fast enough into the air. When it is made narrower, as the figure between the pricked lines, we call it the golden fish, from its manner of acting. For if you take it by the tail, and hold it at a foot or greater horizontal distance from the prime conductor, it will when let go, fly to it with a brisk but wavering motion, like that of an eel through the water; it will, then, take place under the prime conductor, at perhaps a quarter or half an inch distance, and keep a continual shaking of its tail like a fish, so that it seems animated. Turn its tail towards the prime conductor, and then it flies to your finger and seems to nibble it. And if you hold a plate under it at six or eight inches distance, and cease turning the globe, when the electrical atmosphere of the conductor grows small, it will descend to the plate and swim back again several times with the same fish-like motion, greatly to the entertainment of spectators. By a little practice in blunting or sharpening the heads or tails of these figures, you may make them take place as desired, nearer or farther from the electrified plate.

27. It is said in section 8, of this paper, that all kinds of common matter are supposed not to attract the electrical fluid with equal strength; and that those called electrics *per se*, as glass, &c., attract and retain it strongest, and contain the greatest quantity. This latter position may seem a paradox to some, being contrary to the hitherto received opinion; and therefore I shall now endeavour to explain it.

28. In order to this, let it first be considered, that we cannot by any means we are yet acquainted with, force the electrical fluid through glass. I know it is commonly thought that it easily pervades glass; and the experiment of a feather suspended by a thread,

in a bottle hermetically sealed, yet moved by bringing a rubbed tube near the outside of the bottle, is alleged to prove it. But if the electrical fluid so easily pervades glass, how does the vial become charged (as we term it) when we hold it in our hands? Would not the fire thrown in by the wire, pass through to our hands, and so escape into the floor? Would not the bottle in that case be left just as we found it, uncharged, as we know a metal bottle so attempted to be charged would be? Indeed, if there be the least crack, the minutest solution of continuity in the glass, though it remains so tight that nothing else we know of will pass, yet the extremely subtle electric fluid flies through such a crack with the greatest freedom, and such a bottle we know can never be charged: What then makes the difference between such a bottle and one that is sound, but this, that the fluid can pass through the one and not the other?

29. It is true, there is an experiment that at first sight would be apt to satisfy a slight observer, that the fire thrown into the bottle by the wire, does really pass through the glass. It is this: place the bottle on a glass stand, under the prime conductor; suspend a bullet by a chain from the prime conductor, till it comes within a quarter of an inch right over the wire of the bottle; place your knuckle on the glass stand, at just the same distance from the coating of the bottle, as the bullet is from its wire. Now let the globe be turned, and you see a spark strike from the bullet to the wire of the bottle, and the same instant you see and feel an exactly equal spark striking from the coating on your knuckle, and so on, spark for spark. This looks as if the whole received by the bottle was again discharged from it. And yet the bottle by this means is charged! And therefore the fire that thus leaves the bottle, though the same in quantity, cannot be the very same fire that entered at the wire, for if it were the bottle would remain uncharged.

30. If the fire that so leaves the bottle be not the same that is thrown in through the wire, it must be fire that subsisted in the bottle (that is in the glass of the bottle), before the operation began.

31. If so, there must be a great quantity in glass, because a great quantity is thus discharged, even from very thin glass.

32. That this electrical fluid or fire is strongly attracted by glass, we know from the quickness and violence with which it is resumed by the part that had been deprived of it, when there is an opportunity. And by this, that we cannot from a mass of glass, draw a quantity of electric fire, or electrify the whole mass minus, as we can a mass of metal. We cannot lessen or increase its whole quantity, for the quantity it has it holds; and it has as much as it can hold. Its pores are filled with it as full as the mutual repellency of the particles will admit; and what is already in refuses, or strongly repels, any additional quantity. Nor have we any way of moving the electrical fluid in glass, but one; that is, by covering

part of the two surfaces of thin glass with non-electrics, and then throwing an additional quantity of this fluid on one surface, which spreading in the non-electric, and being bound by it to that surface, acts by its repelling force on the particles of the electrical fluid contained in the other surface, and drives them out of the glass into the non-electric on that side from whence they are discharged, and then those added on the charged side can enter. But when this is done there is no more in the glass, nor less than before, just as much having left it on one side as it received on the other.

33. I feel a want of terms here, and doubt much whether I shall be able to make this part intelligible. By the word surface, in this case, I do not mean mere length and breadth without thickness; but when I speak of the upper or under surface of a piece of glass, the outer or inner surface of the vial, I mean length, breadth, and half the thickness, and beg the favour of being so understood. Now, I suppose, that glass in its first principles, and in the furnace, has no more of this electrical fluid than other common matter: that when it is blown, as it cools, and the particles of common fire leave it, its pores become a vacuum: that the component parts of glass are extremely small and fine, I guess from its never showing a rough face when it breaks, but always a polish; and from the smallness of its particles I suppose the pores between them must be exceeding small, which is the reason that aquafortis, nor any other menstruum we have can enter to separate them and dissolve the subject; nor is any fluid we know of fine enough to enter, except common fire, and the electric fluid. Now the departing fire leaving a vacuum, as aforesaid, between these pores, which air nor water are fine enough to enter and fill, the electric fluid (which is every where ready in what we call the non-electrics, and in the non-electric mixtures that are in the air), is attracted in; yet does not become fixed with the substance of the glass, but subsists there as water in a porous stone, retained only by the attraction of the fixed parts, itself still loose and a fluid. But I suppose farther, that in the cooling of the glass, its texture becomes closest in the middle, and forms a kind of partition, in which the pores are so narrow, that the particles of the electrical fluid, which enter both surfaces at the same time, cannot go through, or pass and repass from one surface to the other, and so mix together; yet, though the particles of electric fluid imbibed by each surface, cannot themselves pass through to those of the other, their repellency can, and by this means they act on one another. The particles of the electric fluid have a mutual repellency, but by the power of attraction in the glass they are condensed or forced nearer to each other. When the glass has received, and, by its attraction, forced closer together so much of this electric fluid, as that the power of attracting and condensing in the one, is equal to the power of expansion in the other, it can imbibe no more, and that remains its constant whole quantity; but each surface would receive more, if the repellency of

what is in the opposite surface did not resist its entrance. The quantities of this fluid in each surface being equal, their repelling action on each other is equal; and therefore those of one surface cannot drive out those of the other; but if a greater quantity is forced into one surface than the glass would naturally draw in, this increases the repelling power on that side, and overpowering the attraction on the other, drives out part of the fluid that had been imbibed by that surface, if there be any non-electric ready to receive it: such there is in all cases where glass is electrified to give a shock. The surface that has been thus emptied by having its electrical fluid driven out, resumes again an equal quantity with violence, as soon as the glass has an opportunity to discharge that over quantity more than it could retain by attraction in its other surface, by the additional repellency of which the vacuum had been occasioned. For experiments favouring (if I may not say confirming) this hypothesis, I must, to avoid repetition, beg leave to refer you back to what is said of the electrical phial in my former papers.

34. Let us now see how it will account for several other appearances. Glass, a body extremely elastic (and perhaps its elasticity may be owing in some degree to the subsisting of so great a quantity of this repelling fluid in its pores), must, when rubbed, have its rubbed service somewhat stretched, or its solid parts drawn a little farther asunder, so that the vacancies in which the electrical fluid resides become larger, affording room for more of that fluid, which is immediately attracted into it from the cushion or hand rubbing, they being supplied from the common stock. But the instant the parts of the glass so opened and filled have passed the friction, they close again, and force the additional quantity out upon the surface, where it must rest till that part comes round to the cushion again, unless some non-electric (as the prime conductor) first presents to receive it.* But if the inside of the globe be lined with a non-electric, the additional repellency of the electric fluid, thus collected by friction on the rubbed part of the globe's outer surface, drives an equal quantity out of the inner surface into that non-electric lining, which receiving it, and carrying it away from the rubbed part into the common mass, through the axis of the globe, and frame of the machine, the new collected electrical fluid can enter and remain in the outer surface, and none of it (or a very little) will be received by the prime conductor. As this charged part of the globe comes round to the cushion again, the outer surface delivers its overplus fire into the cushion, the opposite inner surface receiving at the same time an equal quantity from the floor. Every

* In the dark the electric fluid may be seen on the cushion in two semicircles or half-moons, one on the fore-part, the other on the back part of the cushion, just where the globe and cushion separate. In the fore crescent the fire is passing out of the cushion into the glass; in the other it is leaving the glass, and returning into the back part of the cushion. When the prime conductor is applied to take it off the glass, the back crescent disappears.

electrician knows that a globe wet within will afford little or no fire, but the reason has not before been attempted to be given, that I know of.

34. So if a tube lined with a non-electric* be rubbed, little or no fire is obtained from it. What is collected from the hand in the downward rubbing stroke, entering the pores of the glass, and driving an equal quantity out of the inner surface into the non-electric lining: and the hand in passing up to take a second stroke, takes out again what had been thrown into the outer surface, and then the inner surface receives back again what it had given to the non-electric lining. Thus the particles of electrical fluid belonging to the inside surface go in and out of their pores every stroke given to the tube. Put a wire into the tube, the inward end in contact with the non-electric lining, so it will represent the Leyden bottle. Let a second person touch the wire while you rub, and the fire driven out of the inward surface when you give the stroke, will pass through him into the common mass, and return through him when the inner surface resumes its quantity, and therefore this new kind of Leyden bottle cannot be so charged. But thus it may: after every stroke, before you pass your hand up to make another, let a second person apply his finger to the wire, take the spark, and then withdraw his finger; and so on till he has drawn a number of sparks; thus will the inner surface be exhausted, and the outer surface charged; then wrap a sheet of gilt paper close round the outer surface, and grasping it in your hand you may receive a shock by applying the finger of the other hand to the wire: for now the vacant pores in the inner surface resume their quantity, and the overcharged pores in the outer surface discharge that overplus; the equilibrium being restored through your body, which could not be restored through the glass. If the tube be exhausted of air, a non-electric lining, in contact with the wire, is not necessary; for in vacuo the electrical fire will fly freely from the inner surface without a non-electric conductor: but air resists in motion; for being itself an electric *per se*, it does not attract it, having already its quantity. So the air never draws off an electric atmosphere from any body, but in proportion to the non-electrics mixed with it: it rather keeps such an atmosphere confined, which from the mutual repulsion of its particles, tends to dissipation, and would immediately dissipate in vacuo. And thus the experiment of the feather inclosed in a glass vessel hermetically sealed, but moving on the approach of the rubbed tube is explained. When an additional quantity of the electrical fluid is applied to the side of the vessel by the atmosphere of the tube, a quantity is repelled and driven out of the inner surface of that side into the vessel, and there affects the feather, returning again into its pores, when the tube with its atmosphere is withdrawn; not that the particles of that atmosphere did themselves

* Gilt paper, with the gilt face next the glass, does well.

pass through the glass to the feather. And every other appearance I have yet seen, in which glass and electricity are concerned, are, I think, explained with equal ease by the same hypothesis. Yet, perhaps, it may not be a true one, and I shall be obliged to him that affords me a better.

35. Thus I take the difference between non-electrics and glass, an electric *per se*, to consist, in these two particulars. 1st. That a non-electric easily suffers a change in the quantity of the electric fluid it contains. You may lessen its whole quantity, by drawing out a part, which the whole body will again resume ; but of glass you can only lessen the quantity contained in one of its surfaces ; and not that, but by supplying an equal quantity at the same time to the other surface : so that the whole glass may always have the same quantity in the two surfaces, their two different quantities being added together. And this can only be done in glass that is thin ; beyond a certain thickness we have yet no power that can make this change. And, 2dly., that the electric fire freely removes from place to place, in and through the substance of a non-electric, but not so through the substance of glass. If you offer a quantity to one end of a long rod of metal, it receives it, and when it enters, every particle that was before in the rod pushes its neighbour quite to the farther end, where the overplus is discharged ; and this instantaneously where the rod is part of the circle in the experiment of the shock. But glass, from the smallness of its pores, or stronger attraction of what it contains, refuses to admit so free a motion ; a glass rod will not conduct a shock, nor will the the thinest glass suffer any particle entering one of its surfaces to pass through to the other.

53. Hence we see the impossibility of success in the experiments proposed, to draw out the effluvial virtues of a non-electric, as cinnamon, for instance, and mixing them with the electric fluid, to convey them with that into the body, by including it in the globe, and then applying friction, &c. For although the effluvia of cinnamon, and the electric fluid should mix within the globe, they would never come out through the pores of the glass, and so go to the prime conductor; for the electric fluid itself cannot come through; and the prime conductor is always supplied from the cushion, and that from the floor. And besides, when the globe is filled with cinnamon, or other non-electric, no electric fluid can be obtained from its outer surface, for the reason before-mentioned. I have tried another way which I thought more likely to obtain a mixture of the electric and other effluvia together, if such a mixture had been possible. I placed a glass plate under my cushion, to cut off the communication between the cushion and floor ; then brought a small chain from the cushion into a glass of oil of turpentine, and carried another chain from the oil of turpentine to the floor, taking care that the chain from the cushion to the glass touched no part of the frame of the machine. Another chain was fixed to the prime conductor, and held in the hand of a person to be electrified. The

ends of the two chains in the glass were near an inch distant from each other, the oil of turpentine between. Now the globe being turned, could draw no fire from the floor through the machine, the communication that way being cut off by the thick glass plate under the cushion: it must then draw it through the chains whose ends were dipped in the oil of turpentine. And as the oil of turpentine being an electric *per se*, would not conduct, what came up from the floor was obliged to jump from the end of one chain to the end of the other, through the substance of that oil, which we could see in large sparks, and so it had a fair opportunity of seizing some of the finest particles of the oil in its passage, and carrying them off with it: but no such effect followed, nor could I perceive the least difference in the smell of the electric effluvia thus collected, from what it has when collected otherwise; nor does it otherwise affect the body of a person electrified. I likewise put into a phial, instead of water, a strong purgative liquid, and then charged the phial, and took repeated shocks from it, in which case every particle of the electrical fluid must, before it went through my body, have first gone through the liquid when the phial is charging, and returned through it when discharging, yet no other effect followed than if it had been charged with water. I have also smelt the electric fire when drawn through gold, silver, copper, lead, iron, wood, and the human body, and could perceive no difference: the odour is always the same where the spark does not burn what it strikes; and therefore I imagine it does not take that smell from any quality of the bodies it passes through. And indeed, as that smell so readily leaves the electric matter, and adheres to the knuckle receiving the sparks and to other things, I suspect that it never was connected with it, but arises instantaneously from something in the air acted upon by it: for if it was fine enough to come with the electric fluid through the body of one person, why should it stop on the skin of another?

But I shall never have done, if I tell you all my conjectures, thoughts, and imaginations on the nature and operations of this electric fluid, and relate the variety of little experiments we have tried. I have already made this paper too long, for which I must crave pardon, not having time now to abridge it. I shall only add, that as it has been observed here that spirits will fire by the electric spark in the summer time, without heating them, when Fahrenheit's thermometer is above 70; so when colder, if the operator puts a small flat bottle of spirits in his bosom, or a close pocket with the spoon, some little time before he uses them, the heat of his body will communicate warmth more than sufficient for the purpose.

PROFESSOR SCHÖENBEIN'S *Experiments: and his Remarks on* DR. FARADAY'S *Hypothesis on the Causes of the Inactivity of Iron in Nitric Acid.*

(*Continued from page* 41).

THE explanation given by Faraday of the passive condition of iron in nitric acid of certain strength is, in the first place, that the metal becomes surrounded by a thin film of oxide: and secondly, that this oxide is not soluble in such nitric acid. Therefore, according to this view, the sole cause of the inaction would be merely a *mechanical* obstruction; or in other words, the *metallic* iron and the acid do not come into contact with each other. Faraday, also, seems to give a similar explanation to the following fact, which I have observed, viz.: when a voltaic circuit is completed, oxygen gas is liberated at the *positive* iron wire.

To this strange doctrine I must first remark, that the surface of a piece of iron rendered inert in nitric acid of the specific gravity 1·35, exhibits a pure metallic surface, much brighter than by any other mode of cleaning; and no trace of oxide is observable by the closest inspection. I will not, however, dwell upon this topic, although I am aware that it deserves consideration. In one of my former essays, I stated that iron wire made passive towards nitric acid, by any mode whatever, operates like common iron, in other nitric acid considerably diluted: whilst an iron wire acting in the capacity of a positive pole, exhibits an absolute chemical indifference to nitric acid of all strengths. This fact appears to me to argue forcibly against the English philosopher's hypothesis: for if we were to suppose that, at the moment of immersion of the iron wire in the dilute acid, a thin layer of this *questionable* oxide is formed, and that from this circumstance the liberation of gas originates, we cannot conceive how this oxide can remain even a moment in dilute acid without being dissolved in it; that is, in an acid so diluted that, according to Faraday, the oxide cannot remain indifferent and unaltered. If, for instance, the chemical indifference of the iron to the nitric acid were materially influenced by the degree of dilution of the latter, the iron, under the circumstances alluded to, would remain *active*, a nitrate of iron would be formed, and no liberation of oxygen from the metal would occur. Experience, however, proves that precisely the reverse happens, which is exceedingly unfortunate for the hypothesis. It is true, Faraday mentions that iron in nitric acid (without giving us any idea of its strength), is dissolved, even when it operates in the capacity of a positive pole in it. According to my experiments, which were conducted with the greatest possible care, no trace of the metal is dissolved under these circumstances, although the nitric acid be diluted with several times its own quantity of water. I kept an iron wire, which was connected

* Poggendorff's "Annalen der Physik and Chemie," b. xxxix.

with the positive pole of a *couronne des tasses* of fifteen cups, for many hours in such an acid liquor; after which not the slightest trace of iron could be detected in it. If, however, another acid of the common strength, of the s. g. 1·35, for instance, be added, the result becomes modified, and a little oxide of iron is always to be found in the liquor. But I do not consider that this oxide is formed in the acid; it appears to be produced on that part of the wire which is above the surface of the acid, by the acid vapours which are continually rising; and the nitrate thus formed is conducted to the acid below by capillary attraction.

A more important fact to which I must now solicit attention is, that the iron wire which dips into the diluted acid and is indifferent to it, becomes acted on the moment the electric current ceases to be transmitted through it. If, for instance, we were to allow the iron wire to remain in the acid to be examined, and then to interrupt the electric current, immediately a dark yellowish brown matter, which is nitrate of iron, forms around the wire. From these facts it appears that the most essential cause of the chemical indifference of iron to nitric acid, is due to the influence of the electric current, and not any surrounding thin pellicle of oxide, nor to any certain quantity of water in the acid. It is obvious, also, that if the indifference of the positive iron wire was owing to a thin enveloping oxide, the same wire ought to remain passive when separated from the pole and immersed in common nitric acid: such, however, is not the case.

The circumstance that the positive iron wire is acted upon similarly in other dilute acids as in the nitric acid is rather unfavourable to Faraday's hypothesis. It is known that iron becomes completely passive by merely dipping it into fuming nitric acid: by what means then, is the pellicle of oxide formed in such cases? I can see no means but by decomposition of the nitric acid, because no other appears to be possible. I must doubt, however, that such an action takes place; and if it does not, it would be difficult to ascertain the origin of the iron's oxidation. I will here state that the galvanometer indicates a feeble electric current when the iron is dipped into highly concentrated nitric acid: but this does not prove the oxidation of the metal. I will add one other remark, which I think is not unimportant in this subject. In my last paper I mentioned the action of an acid of 1·35 upon the iron, which took place by sudden fits, which I stated was occasioned by the metal becoming active and passive alternately. Faraday would explain this phenomenon on the supposition that at one instant a film of oxide is formed around the wire, and thus protects it from the action of the acid; and the next moment the oxide is dissolved in the acid, and the metal exposed to its action. But such an explanation forms a contradiction in itself; because it first supposes the film of oxide to be *insoluble*, and afterwards *soluble* in the nitric acid, which is an absurdity. Moreover, one might propose the following unanswerable question. Why is iron rendered passive by frequent immersions in

nitric acid; and therefore, why is an oxide formed which, in the first place enters into combination with the acid, but afterwards remains in the acid an insoluble oxide. The whole of the facts lead me to suppose that Faraday's views respecting the passive state of iron do not give a satisfactory explanation.

Bâle, October, 1836.

(To be continued in the next number).

The Electric Column considered as a Maintaining Power, or First Mover, for Mechanical Purposes. By GEORGE JOHN SINGER, *Esq.**

THE power of the electric column as a source of mechanical action was first discovered and applied by that excellent philosopher M. De Luc, the admirable inventor of that important instrument; and it is to his active discrimination and unceasing exertions we are indebted for the principal mechanical arrangements which have been employed to render the variable action of the column equal to the production of a constant though unequal motion.

The principal object of such an attempt is to enable an observer to measure the actual variation in the power of the column at different times, and under dissimilar circumstances; and, by a comparison of these changes with the usual meteorological phenomena, to ascertain if any connexion can be traced between the spontaneous electricity of the column, and the natural electricity of the earth and the atmosphere.

For this purpose any arrangement may be employed which is capable either of producing or maintaining the motion of light substances by the immediate action of the column; and that will be most eligible which produces this effect most certainly, and by the least complex means.

With columns of small power, the frequency with which the leaves of Bennet's electrometer are made to open and strike the sides of the glass, during their contact with one extremity of the column, for a certain number of seconds, becomes a measure of the comparative power of the instrument at different times: but its distinct expression is prevented by the tendency of the gold leaves to stick to the sides of the glass; and this arrangement is therefore by no means fitted for permanent observations.

When an insulated conducting substance is freely suspended between two balls, or bells, connected respectively with the opposite ends of the column, I have found that motion is constantly produced if the weight of the pendulum, and the distance of the bells, are exactly proportioned to the acting power of the column at its mean rate of intensity: but if these circumstances are not strictly attended to, the motion will soon cease; and the want of complete success in the original experiments of M. De Luc and of Mr. B. M. Forster, most probably arose from this cause; for, in the construction of a

* From Tilloch's "Philosophical Magazine."

number of instruments on this plan, I have had but one failure, and in that instance the apparatus was finished in such haste as to preclude a proper attention to the circumstances above stated.

Fig. 1. Plate 3, represents the arrangement of my Electric Chime. A series of about 1600 groups of zinc, silver, and paper discs, are disposed in two columns, separately insulated in a vertical position; the positive end of one is placed lowest, and the negative end of the other, their upper extremities being connected so as to form in effect one series, having at each of its extremities a small bell; between the bells a small ball is suspended by a thread of raw silk, so as to hang at an equal and very small distance from each of them if unelectrified. The action of the column occasions this ball to vibrate between the bells and produce an electric chime, in which the variable action of the instrument at different times is indicated by an increased or diminished velocity of ringing. There is a circular groove in the base of the instrument which receives the rim of a glass shade, by which dust and moisture are prevented from impeding its action.

Fig. 2 represents a convenient modification of the arrangement devised by M. De Luc, and to which he has given the name of Aërial Electroscope. It is constructed nearly in the same manner as the chime, but has balls at its lower extremities instead of bells. From the positive end a wire W, proceeds upwards a few inches parallel to the column, and is then bent into a hook to serve as a support to the pendulum, which consists of a fine silver wire to which a gilt pith-ball is attached. This pendulum, being in conducting communication with the positive extremity of the column, will necessarily recede from it and approach the opposite ball; but it is prevented from actual contact with that ball by a brass fork F, across which a very fine silver wire is stretched. This wire discharges the electricity of the pendulum, and at the same time produces a kind of jerk which prevents the pith-ball from sticking: the pendulum now falls again into contact with the positive ball, but becoming again electrical, recedes from it, and again strikes the cross wire; and in this way, if properly constructed, may continue its vibrations for an unlimited period.

I have sometimes made a variation in this apparatus, by removing the cross wire and the conducting support of the pendulum, and by substituting for it a pith-ball suspended by a silk thread, and accurately proportioned in weight and size to the medium power of the column. By this means the motion occurs over more space than in either of the preceding arrangements, and is therefore more obvious, and well calculated for observation, as the irregularity is considerable, and may be noticed when the temperature of the surrounding medium varies but slightly.

During my employment of the very extensive series of columns I have constructed, I have frequently attempted to produce a rotatory motion by the direct action of their electrical power, but hitherto the attempt has continued unsuccessful; by indirect means, however,

the same object has of late been very ingeniously obtained. In October last, my friend Mr. Lightfoot, a very active philosopher, who has made many interesting observations on this subject, first suggested the employment of an inflexible pendulum as a means of converting the reciprocating motion usually produced by the column into a source of rotatory movement; and the correctness of this idea was soon afterwards practically verified by my pupil Mr. F. Ronalds, who with the assistance of a watchmaker, has made a very successful and truly ingenious arrangement, by which a simple and curious electrical clock is produced.

The rotatory motion obtained by this indirect means, is however rather curious than useful; for it is scarcely so correct an indication of the power of the column as the simple pendulum, and requires a much more extensive series to keep it in motion; it cannot therefore be preferred for the usual purposes of observation, and has I fear very little chance of becoming at all useful as a time-keeper; for the variable action of the column must render it a most irregular maintaining power, which it will be very difficult, if not impossible, to correct effectually.

The most elegant and at the same time the most simple movement yet produced by the action of the electric column appears to be that employed by Signior Zamboni, who has made some interesting discoveries on the general structure of the instrument. He employs a vertical needle supported by a delicate pivot or knife-edge a little above its centre of gravity, the position of which may be readily altered by means of a sliding weight attached to the lower extremity of the needle, which may by that means be so adjusted as to possess the properties of an accurate scale-beam, and will maintain its oscillations over a considerable space by a very slight impulse.

The upper end of the needle, for at least an inch, is formed of varnished glass; and on this a ring of gold, or a gilded ball of pith or cork, is fixed; the axis of the needle is supported midway between two vertical columns insulated, but connected together at the bottom, so that the upper ends of the columns become the positive and negative extremities of the series; the upper and insulated extremity of the needle comes in contact alternately with each of these ends, receives its electrical state, and recedes towards the other, where the same process ensues; and thus the vibrations of the needle are maintained with great constancy over a considerable space.

Fig. 3 represents the form I have employed for this construction: the needle is supported by a brass arm which slides on one of the columns; it is suspended by a delicate pivot, and has at its summit a fine varnished glass tube, to which a gilded ball is affixed; the lower extremity of the needle is provided with a sliding weight, by which the relation of the centre of gravity to the point of suspension is accurately adjusted; to render the contacts perfect, and least liable to change, the gilded ball does not strike the brass caps of the columns, but touches alternately two gold wires connected with them.

In this construction the needle is not moved by the direct attraction of the column; but being once put into a state of vibration, its motion, which would naturally decline, and finally terminate by the operation of friction and by the resistance of the air, is renewed at each contact by the impulse of electrical attraction, which is alternately exerted on the needle in opposite directions by each extremity of the column; and as this attraction does not sensibly act on the pendulum until it is very near the attracting surface, its operation commences when it is most wanted, and, without materially affecting the action of the pendulum in any other way, occasions it to describe constantly equal arcs at every vibration.

It is obvious, that by connecting a proper lever and ratchet-wheel with the axis of the needle, motion may be readily communicated to indexes, or to other wheels; and this I am informed has been done during the past year, by some experimentalists on the continent. I have since tried the experiment, and find it succeeds perfectly, but requires a more extensive series to overcome the increased friction.

An effect very nearly resembling the action of the beam of a steam engine may be produced by placing the needle in a horizontal instead of a vertical position. For this purpose it should be constructed in the same manner as an ordinary scale-beam; having equal arms terminated by gilt balls, and its point of suspension above its centre of gravity. If a needle of this kind be insulated, and placed with one of its balls a few inches above the positive extremity of a powerful column, whilst the opposite ball is similarly situated with respect to the negative extremity, it will, when once put into a state of oscillation, continue to move with considerable regularity, and with a momentum which renders it probable that, by the application of a proper mechanical arrangement, a tolerably regular source of rotatory motion would be obtained.

I have now completed a series of columns comprising upwards of 60,000 groups of a peculiar and powerful arrangement, but have not as yet combined them so as to institute any accurate experiments on their effects; but I trust it will not be long ere I have leisure to accomplish this object. G. J. Singer.

London, May 1, 1815.

SUPPLEMENT TO THE ANNALS

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ELEMENTARY LECTURES ON ELECTRICITY.

LECTURE XIX.

Mr. Stephen Grey, a Charter-house pensioner, who discovered the difference between conductors and non-conductors, and other capital facts, appears also to have been the first electrician who entertained the notion of an identity between electricity and lightning. In the concluding paragraph of one of Mr. Grey's papers, printed in the "Transactions of the Royal Society," the author says, whilst contemplating the results of the experiments he had been describing in the same paper—

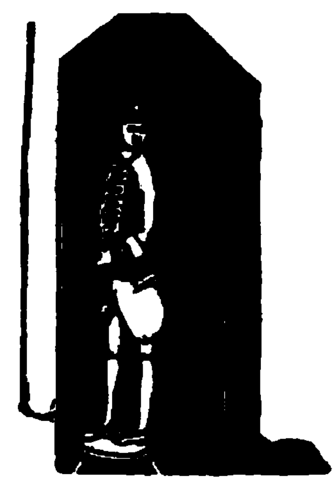
"By these experiments we see, that an actual flame of fire, with an explosion, and an ebullition of cold water, may be produced by communicative electricity (communicated to a metallic rod, an iron ball, and other bodies, on which he had been experimenting): and though these effects are at present but in *minimis*, it is probable that in time there may be found out a method to collect a greater quantity of it, and consequently to increase the force of this electric fire, which, by several of these experiments seems to be of the same nature with that of thunder and lightning."

These predictions, which were printed in the Transactions for the year 1735, ten years prior to the discovery of the Leyden jar, and about seventeen before the first successful experiments on atmospherical electricity, were wonderfully verified in these two memorable events.

After the wonderful powers of the Leyden jar had become generally known, there can be no wonder of the identity of electricity and lightning being suspected by those electricians who paid so close attention to the phenomena; indeed, it soon became a prevalent opinion: but it is certainly to Dr. Franklin that the honour is due of earnestly calling the attention of philosophers to this topic

by his admirable observations on the analogies which the effects of lightning and electricity present, and also of placing before them a plan by which a satisfactory experiment may be made. Dr. Franklin's plan of experimenting bears date 1749, and the following description of it is in his own words:—

"To determine the question whether the clouds that contain lightning are electrified or not, I would propose an experiment to be tried where it may be done conveniently. On the top of some high tower or steeple place a kind of sentry box, as in the figure, big enough to contain a man and an electrical stand. From the middle of the stand let an iron rod rise, and pass bending out of the door, and then upright twenty or thirty feet, pointed very sharp at the end. If the electrical stand be kept clean and dry, a man standing on it when such clouds are passing low, might be electrified and afford sparks, the rod drawing fire to him from the cloud. If any danger to the man should be apprehended (though I think there would be none) let him stand on the floor of his box, and now and then bring near to the rod the loop of a wire that has one end fastened to the leads, he holding it by a wax handle; so the sparks, if the rod is electrified, will strike from the rod to the wire, and not affect him."



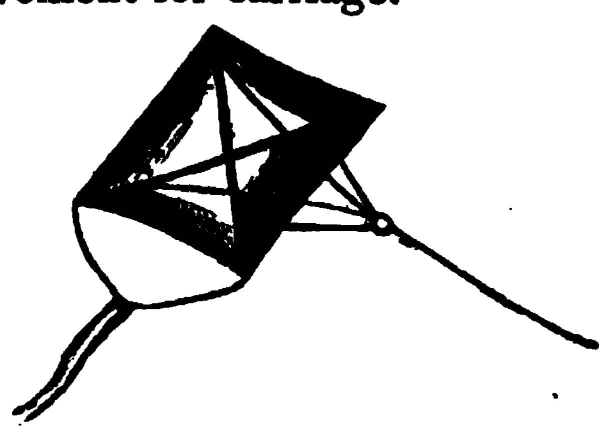
The plan for this grand experiment being made generally known throughout Europe and America, many philosophers of both countries made preparations for carrying it into execution. The French were the first in the field on this memorable occasion, and M. Dalibard's apparatus, erected at Marley-la-ville, had the honour of being the first that was visited by the "ethereal fire," though that philosopher himself, in consequence of an absence from home at the time, was deprived of the glory of being the first beholder of it, the enviable good fortune falling to the lot of his servant Coiffer, who was left in charge of the apparatus.

Dalibard's apparatus was similar to that proposed by Franklin, which, however, was not placed on a steeple, but on some high ground. It consisted of an iron rod forty feet long, the lower end of which was brought into the sentry box, where the rain could not come. On the outside the rod was fastened by silken cords to three stout wooden posts firmly fixed in the ground. On Wednesday, the 10th of May, 1752, between two and three o'clock in the afternoon, Coiffer saw the first electric spark drawn from the atmosphere ever witnessed by man. He heard a clap of thunder at some distance, and on applying a small Leyden bottle to the iron rod, electric sparks were obtained, and the great question set at rest about one month earlier than Franklin himself had an opportunity of making a satisfactory experiment, which he did by means of an elevated kite in June of the same year, and without having any information of what had been done in France.

Franklin's kite was simply a silk handkerchief stretched diagonally by two sticks, with the usual loop, tail, and hempen string, which was insulated by means of a silken cord at the lower end.

The first indication of electric action observed were the repulsions among the fibres of the string, which stood erect and avoided one another as if attached to the prime conductor of a machine. Shortly after these appearances, Franklin obtained sparks from a key which he had hung at the lower end of the hempen string. A dense cloud was passing over the kite at the time, and in consequence of some rain which fell and wet the kite string, and thus made it a better conductor, the electric fluid was copiously exhibited in various ways. From the time that Franklin's kite experiments became known, to the present, an electric kite has been considered as an indispensable apparatus for explorations of the atmosphere.

The electric kites that I employ differ little from that first used by Franklin, excepting in the manner of applying the string, the tail, and in making them portable and convenient for carriage.



I provide a square of sarsnet, and a pair of stretchers made of light wood, and well varnished for protection against the wet. These stretchers are coupled together by means of a pin which passes through the centre of both, and on which, as a pivot, they can turn and be set to the proper angle for stretching the silk, or they can be brought close and parallel together. One extremity of each stretcher is fastened permanently to the two upper corners of the silk, and each of the other extremities is furnished with a projecting wire loop or staple, which passes through an eye-hole in the lower corner of the silk, the end of the stretcher itself forming a shoulder, which prevents its passing through also. By these means the silk can be stretched or furled at pleasure. The tail consists of several lengths of calico ribands, linked end to end by means of hooks and eyes, its upper end being attached to the middle of a tape, which by means of a hook at each end, can be attached to, or detached from, the lower corners of the kite at pleasure.

In order to relieve the stretchers as much as possible from the strain of the wind's force, the surface of the silk is divided into two equal areas, a central area, and a marginal area, so that the strain on the central and outer parts of the stretchers shall nearly balance one another. This is accomplished by means of four braces, two to each stretcher, and attached at a proper distance from their extremities, to insure the square area within the four points to which the braces are attached, to be equal to the marginal area without. The braces, after passing through small holes in the silk, have their outer extremities united in a brass ring, and to this ring the kite-string, containing a wire strand, is attached by means of a hook. By this

arrangement the wind blows the silk against the stretchers with but little chance of breaking them: and the four braces keep the kite much steadier than by the usual single loop. There are some other peculiarities belonging to this kite, but they are hardly worth describing, excepting, perhaps, the advantage of having the tail in several pieces, which is found convenient to give the proper trim for various forces of the wind.

The complete stock of my atmospheric electrical apparatus, in addition to five silken kites with their wired strings, consists of a reservoir, a Leyden bottle with its discharger, a compass-needle, a soft steel needle enclosed in a spiral wire, and a delicate pith ball electroscope, which, with the exception of the reservoir, are sufficiently portable to be put in the pocket.

The reservoir consists of a hollow tin cylinder, mounted on a stout glass pillar, as represented in the figure opposite. The upper end of the tin cylinder is open for the purpose of lodging in it that part of the string not taken up by the kite, and in order to keep the strain of the string from pulling down the reservoir, the latter is anchored by silken cables, which keep it steady; the string, also, has a silken cord termination, which reaches over the mouth of the prime conductor, and has its other end anchored in the ground. To the lower part of the reservoir is screwed a Lane's discharger, with a wire for occasional connexions.

The coated bottle is enclosed in a cylindrical brass case, to protect it from breaking. When the cover is taken off the body of the case the neck of the bottle, with its brass ball, is exposed. A small discharging rod, with one metal branch and a short ivory handle, is attached to one end of a copper wire, the other end being connected with the brass case, and consequently with the outer coating of the jar. Within the lid of the cylindrical case is the compass card and its magnetic needle, which being covered with glass, in the usual way of fitting up small compass boxes, is protected from injury, and when removed from the case is placed horizontally on the ground, and the needle takes its proper position. The jar, with its neck and ball

exposed, and the discharger applied as in the act of discharging the bottle through the spiral wire, is represented by the figure. The lid of the case in the capacity of a compass-box is seen below the jar.

When the kite is about to be used on a fine cloudless day, for the

mere purpose of ascertaining the character of the electricity of the air at a considerable altitude, it may be let fly from the hand in the usual way, by paying out the string as fast as it can be taken up. When sufficiently high, a *single hitch* is taken round the reel, or stick, if wound on one, to prevent more string leaving it. The silken cord is now fastened to the kite-string, and the other end anchored in the ground. In cases of this kind there is no need of the reservoir. When the kite has been anchored a few minutes, the knuckle may be presented to the string, and probably a spark will be experienced. The bottle is now to be charged by applying its ball to the string, and afterwards discharged by the proper apparatus. If the charge appears high, the spiral with its enclosed needle is to be placed in the circuit as shown in the figure, and a few discharges sent through it. These will magnetize the needle: which, when presented to either pole of the compass needle in the box, will display the character of the pole presented, and this polarity of the magnetized needle will indicate the direction of the electric current through the spiral wire, according to the law of electro-magnetism already explained. If the current traversed the spiral wire *from* the inside *to* the outside of the bottle, its inside was electro-positive, and consequently the string was electro-positive, and also the air was in the same state with reference to the ground. This is the usual electric state of the atmosphere when perfectly clear and no appearance of clouds. Such an atmosphere, however, though constantly positive with reference to the ground, is much more powerfully so on some occasions than on others. I have usually found it most powerful during the sharp cold north-east winds in March and the beginning of April.

With about half a mile of string out, during a sharp breeze from the north-east, I have had a series of sparks, too rapid to be counted, through a plate of air between the reservoir and the ball of the discharger, of an inch and a half in thickness. This circumstance occurred at the Military College at Addiscombe, in March, 1824.

During the afternoon of the same day I attached the lower end of the kite-string to the back-band of another kite, and by this means got up about another quarter of a mile of string. The upper kite now floated very high, and being of a light blue colour, nearly corresponding with the colour of the sky, required a good search of the eye to find it. When the lower string was anchored the shocks which it delivered to those who approached it were exceedingly severe. About fifty of the gentlemen cadets received shocks from the string, but not more than two or three of them could be prevailed on to approach the string a second time. I experienced one of these shocks myself, and the blow was tremendous and general throughout the whole system, but most severe in the arm that received the discharge, the chest, thighs, and shin-bones. I next brought out a large Leyden jar of the capacity of three gallons, and applied its ball to the discharger of the reservoir. The charge of the jar was rapidly accom-

plished, with such an intensity as to occasion spontaneous discharges over the top. Under those favourable circumstances I made a great number of experiments. A piece of ordnance was several times fired by discharges from the jar, and twice by sparks from the string; all the sewing needles that could be mustered were magnetized; copper and silver were revived from their solutions, and water was operated on, but only very slightly decomposed.*

I will just mention in this place, once for all, that I have occasionally found it necessary to elevate the upper end of the string by the assistance of three kites in series, in order to get even a trifling charge of the small experimenting jar.

If the kite experiments be intended to ascertain the electric state of the atmosphere at different altitudes, then it will be necessary to have four or more kites elevated with different lengths of string, in order that they may float at different altitudes. Experiments for this purpose can never give satisfactory results only under a cloudless sky, and then in all cases, as I have before stated, it will be found that the atmosphere is more and more electrical as the strata explored are more elevated.

In hot sultry weather, and especially when hazy, the atmosphere is highly charged with the electric fluid at a very low altitude. I have on some occasions found the shocks from the kite-string quite insupportable when the kite was not higher than a church steeple, and this too when the string was not insulated. Under these circumstances it is impossible to let out much string in the usual way, by paying it through the hand.

When the electric shocks are thus powerful from low altitudes, and it is desirable to get the kite higher, the best method is to bring down the kite, and when down stretch out on the ground the whole length of string intended to go up, with the kite attached at one end and the insulation perfect at the other; also the spare string in the reservoir and the ball of the discharging piece adjusted to a moderate distance, about an inch and a half from it, with its wire stuck in the ground. Thus prepared the kite will ascend from the hand, and when elevated a while, the apparatus at anchor may be observed. If sparks be seen between the reservoir and the discharging ball the power is great, too great indeed for the operator to approach the string. If no sparks be seen the ball may be pushed a little closer until they appear, which, under the circumstances mentioned, are likely to be copiously produced. In some instances during hot hazy days, I have seen the sparks strike through more than two inches in rapid succession for more than an hour continuously. It is easy to charge a jar on such occasions, and by magnetizing a needle, to ascertain the electric state of the haze, which I have always found to be positive.

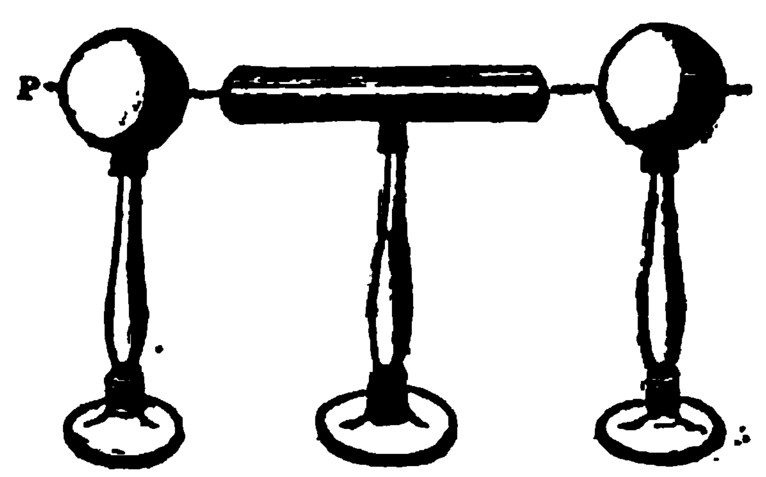
* This series of experiments have been regularly alluded to in my lectures, but never before recorded; nor do I know of any similar series of experiments recorded by any other person.

During the summer season it is always difficult, if not dangerous, to elevate the kite when clouds are about, without the precaution of first stretching the string on the ground and making the other preparations already named: for want of such precaution I have frequently experienced severe blows whilst paying out the string. When there is any appearance of lightning, even though not near, the string must never be let pass through the hand whilst elevating the kite. Flashes of lightning invariably produce electric *waves* in the air to a great distance on every side, and these waves produce tremendous discharges through the medium of the kite string when it happens to be in their way, and might injure or even kill the operator were he close to the apparatus at the time.

Floating clouds also, when no lightning is present, are invariably productive of electric waves, when they are highly charged. I have had a good deal of experience amongst electric waves thus produced, and occasionally have permitted others to experience their effects.* I have frequently been much annoyed by powerful shocks from waves whilst taking in the kite string. On one occasion I was struck a violent blow by a discharge which passed over about two yards of silk ribbon, though the kite string was uninsulated at the time, being tied to a tree. The cloud producing the wave was thin and of small dimensions, and not within a quarter of a mile of the kite.

To give an idea of the manner in which electrical waves are formed, and of their influence on bodies amongst which they flow, it will be necessary to call to your recollection the illustrations already given on the subject of electro-polarization. You are aware that an already charged body has the power of disturbing the natural electric equilibrium of those bodies which are placed within its sphere of influence; and that an electro-positive body repels the fluid *from the vicinal part* of the body on which it acts, and thus renders it negative; and the body itself, taken as a whole, becomes electro-polar whether insulated or not.

You may now suppose the polarizing body to be an insulated sphere as represented by P in the opposite figure, and that the nearest body is a brass cylinder, and beyond the cylinder another metallic sphere N is placed, all insulated. Now, accordingly with the doctrine of electro-polarization, the electro-positive sphere P polarizes the cylin-



* Serjeant Rudd of the Royal Artillery, if still alive, remembers well the effect of an electrical wave. Having presented his hand to the kite string several times without experiencing even a spark, in the Artillery Barrack grounds at Woolwich, he began to laugh at the idea of electric shocks from

der, rendering the vicinal end *negative* and the remote end *positive.* The cylinder now beomes a polarizing body, as decidedly as the original electrical sphere P, and in its turn polarizes the sphere N, by repelling its fluid to the remote side.

In all cases of electro-polarization there is a polar axis, which is a line joining those two opposite points of the polar body on which the electric action is exerted to the highest degree. These points are in fact, the real poles of the body. The polar axis of a sphere passes through its centre, and consequently is coincident with a diameter. When the three bodies represented by the figure, have their centres in the same right line, the polarizing axis of the system passes through the centres of all the bodies, as represented by the dotted line.

Now, in order to produce electric waves in the cylinder and the sphere N, we have only to move the polarizing sphere P to and fro, in, or nearly in the axis of polarization represented by the dotted line. Let us suppose, for instance, that the sphere P, is first placed too remote from the cylinder to disturb its natural equilibrium: under these circumstances, both the cylinder and the sphere N will be neutral. Now advance the sphere P gradually towards one end of the cylinder: a corresponding departure of the fluid takes place from the vicinal to the remote parts of the latter, and the gradual accumulation at the remote end produces a corresponding and simultaneous movement of the fluid belonging to the sphere N. In each body, therefore, there is an *electric wave* during the advance of the disturbing body P. Now withdraw the sphere P: the fluid in both the cylinder and the sphere N flows back again, and the electric waves in those bodies are in the reverse order to the former. It will now be quite obvious that were the sphere P moved to and fro amongst a promiscuous group of bodies, that electric waves would be produced in the group corresponding with the motions of the disturbing electrical sphere.

Hitherto we have considered the bodies to be insulated, and their centres all in the same right line; but we have seen in a former lecture that if the sphere N were uninsulated the polarization would take place with greater facility and to a greater extent; and if instead of moving the polarising sphere P in a line with the centres of the other two bodies, the axis of polarization, and consequently the poles of those bodies, would be very differently situated. If the polarizing body P were to move past or over those other two bodies, the axis of polarization would be continually changing its position. Such also, would be the case were the sphere P to move over an uninsulated mass of any conducting matter whatever, as is the case in

the air. Shortly, however, I spied a cloud making its appearance behind the Repository, and on its approach asked the serjeant to try again. He did so, but before he got his hand near to the string a discharge struck it, and sent the sceptic reeling, to the great amusement of his brother non-commissioned officers who were present.

nature; for when a highly electrized cloud is floating over a tract of country, it polarizes the land as it passes over it, and produces electric waves within the surface, when of good conducting quality, both in the direction of its path and laterally, on both hands, as decidedly as a vessel under sail produces waves on the surface of the water otherwise perfectly at rest.

But it has already been shown that in the electro-polarization of conducting bodies, the intervening plates of air are also polarized, and consequently highly charged clouds polarize the atmospheric air all around them, repelling the electric fluid to a great distance, and leaving a vicinal negative space on every side. Now imagine the cloud to move on, the air around its path successively yielding to its polarizing influence, suffers its natural share of electric fluid to flow to remoter parts, and the advancing cloud, thus endowed with power, enforces an atmospherical electric tide, in correspondence with its progress through the air. It is this primary *tide-wave* in the air that polarizes the ground, and produces a corresponding electric wave over the tract of country above which the cloud floats.

When an electric cloud is driven by the wind towards a high piece of ground whose substance is a bad conductor, that substance resists the electric wave, and will not suffer the fluid from the air to transpierce it: an accumulation then takes place on the face of the hill, which becomes charged, in the manner of charged non-conducting solids generally. The consequence is a reaction against the cloud, upon the principle of electric repulsion. The cloud being now under the influence of two forces, the wind and the electric repulsion, will have its speed retarded, if not arrested altogether. Its future path will depend on the relative power of the two forces, and on the direction lines in which they are exerted. Chatham Lines, which is a portion of the great chalk formation, is remarkable for giving new directions of motion to approaching electric clouds. Shooter's Hill, also, I have known to give electric clouds very different directions to those previously pursued from the force of the wind alone. It is far from being an uncommon circumstance to see electric clouds floating in opposition to a light wind; and very frequently indeed, their motions are oblique to it.

Now, since electric clouds travel with the greatest facility over a country which offers the least resistance to the grand electric tide wave, there can be no wonder at their greater tendency to pass over wet land, rivers, &c., than over dry land, which is a worse conductor, especially when such land is high. When clouds are deflected from the wind's direction, they are certain to be guided by the conducting character of the country below. With respect to Chatham Lines and Shooter's Hill, I have had frequent opportunities of observing clouds deflected by them to the respective neighbouring rivers, the Medway and the Thames. From these facts there seems to be a possibility, at least, of forming an idea of the character of the geo-

logical strata by observing the motions of thunder clouds and others highly charged with electricity.

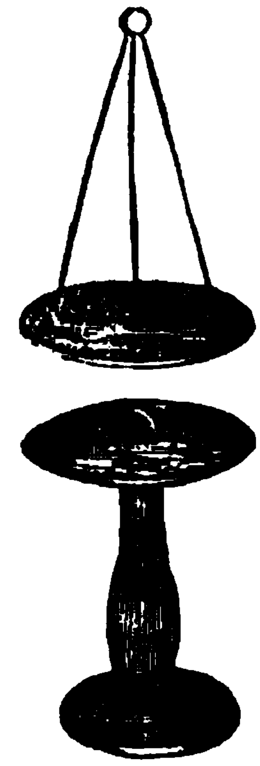
That flashes of lightning produce electric waves on every side, may easily be understood by considering what would happen to the cylinder, were the sphere P to be suddenly charged and only for a moment. A sudden and momentary polarization would be the consequence, by an electric wave *from* the vicinal to the remote end of the cylinder, which would immediately retire again on the exit of the disturbing force from the sphere P. Electric waves produced by flashes of lighting, are necessarily rapid, and only of momentary existence, whilst those produced by clouds are slow and of long continuance. Both classes of electric waves may be illustrated by the following experiments.

Place three or four gold leaf electroscopes in a row, at some distance from one another, on the table, then take hold of the coating of a highly charged Leyden jar, and pass its ball slowly over the electroscopes: the wave thus produced in the air causes their leaves to diverge as this artificial cloud passes over them in succession. If, instead of the usual ball of the Leyden jar, a large well-polished ball were attached to it by a long metal stem, the effect on the electroscopes would indicate the power of this class of waves in a very beautiful manner.

To illustrate electric waves produced by lightning, I place a gold leaf electroscope at a considerable distance from the prime conductor of the machine, and, before the latter becomes electrized, I hold a large brass ball against it. Now turn the machine: nothing happens to the electroscope, but the moment I remove the ball it receives a spark; in fact, a miniature flash of lightning; and the gold leaves of the electroscope are thrown open, indicating the influence of the momentary wave. In neither case is there left any trace of electric action in the electroscopes. But if each electroscope were to be furnished with a pointed wire, projecting upwards, the whole would remain electrical: the series of electroscopes over which the electric ball passed would show that tall rods pointing into the air receive electric fluid whilst an electric cloud passes over them; and the electroscope charged by the wave from a spark is a good illustration of the electrization of tall pointed rods by a wave from a flash of lightning.

Although I have employed the ball of a Leyden jar in these experiments, I am far from supposing that a charged jar is a just resemblance of an electric cloud; nor do I entertain the idea that the air becomes charged in the manner that coated glass is charged. In my opinion, there is not at the present day a more palpable, certainly not a more popular error, amongst writers on electricity, than that of supposing the air to be charged like glass. About the year 1755, an experiment was established by Æpinus and Wilche, at Berlin, which was supposed to show the identity of charged glass and charged plates of air; and although the apparatus was neither more nor less

than a condenser on a large scale, the identity was supposed to be proved by means of it; and from that time to the present the same opinion has prevailed, even amongst the most famous of electricians. This Berlinean experiment is, however, exceedingly interesting, from its affording a better exemplification of lightning than any other. The apparatus for this experiment consists merely of two circular boards, of four or more feet diameter, both of which are covered with tinfoil. When used, one of the boards is suspended by three silken cords to the ceiling, and connected by a wire with the prime conductor. The other is placed, uninsulated, directly beneath the former, their planes being parallel to each other. The adjoining figure will represent their relative situations. When the machine is put into motion the upper board necessarily becomes electro-positive, and upon the principles of polarization, the lower board becomes electro-negative: and the apparatus is now in the capacity of a condenser. If the action of the machine be now arrested for a moment no charge is to be found in the intervening plate of air, nor any spark discoverable from the charged board, beyond such as is usually found at the prime conductor after the machine has ceased working, but coated glass would have retained the charge discoverable by these means!!!



If now, we place a well polished metallic hemisphere on the middle of the lower board, a series of sparks will strike it from the upper one; and when the distance is such that the sparks are not frequent, each spark is highly imitative of a flash of lightning, and the noise, though feeble, is the thunder accompanying the miniature flash; for lightning is an electric discharge in the atmosphere, sometimes between a cloud and the earth, but more frequently amongst the clouds themselves. The electric fluid thus discharged flies swiftly through the air, in which it leaves a vacuous track behind; but of momentary duration only: for the displaced air suddenly collapses, and the noise of thunder, as one sudden report, is produced; which, by reverberation amongst the clouds and neighbouring hills, is echoed and re-echoed many times over in a succession of peculiar sounds, of a gradually decreasing intensity, until the last murmur terminates the electro-acoustic event.

The cause of lightning clouds has long been a topic of speculation amongst philosophers. The celebrated Volta, of Como, showed that the vapour of water from the surface of the earth takes up an immense quantity of the electric fluid, and consequently charges the air with it. To illustrate the fact of vapour or steam taking up electric fluid, I have only to place a small tin dish containing water on the cap of the gold-leaf electroscope, and then put a red-hot cinder into the water. This produces a copious evaporation, and

the gold leaves diverge. Whilst thus divergent I test the electric state of the electroscope, and find it negative. Hence it is obvious that a portion of its natural electricity has flown off by means of the steam. This is the simple fact, and nothing more, and gives no reason whatever why the steam should take away more fluid than naturally belonged to that portion of the water from which it was formed. By the help of Franklin's can and chain, however, we shall be enabled to arrive at a satisfactory explanation.

When the dish of water was at the common temperature it occupied a certain space, and contained its natural quantity of the electric fluid, or precisely that quantity which, in the character of water its susceptibility of charge would allow; but as soon as it became converted into steam, its dimensions expanded, and a corresponding expansion and consequent attenuation of the electric fluid took place, in precisely the same manner as in Franklin's chain when lifted out of the can. This attenuation of electric action in the steam rendered it negative, and being in contact with the unevaporated portion of the water absorbed electric fluid from it, and thus rendered it negative.

The next step in the illustration is to shew that the vapour thus produced does absolutely carry up with it more of the electric fluid than naturally belongs to it when in the state of water. For this purpose I suspend, by means of a silken thread, a hemispherical tin vessel over the steam which rises from the water in the dish, in the manner represented in the figure. The rim of this inverted vessel is turned inwards, and formed into a channel for the purpose of collecting the water from the condensed steam. By this means I not only collect steam but its contained electric fluid also, which condenses as the steam condenses in the inverted vessel. When the evaporation and condensation has proceeded till the gold leaves of the electroscope below have diverged sufficiently for the purpose, I remove the insulated vessel to another electroscope, which immediately displays electric action; and by the application of an excited stick of sealing-wax this action is found to be positive.

The results of these experiments are very interesting in more ways than one; as they not only prove that steam is capable of absorbing more of the electric fluid than the water from which it is formed, but also furnish us with a satisfactory explanation of the origin of the electrization of clouds, which are well known to be masses of the condensed aqueous vapour which had ascended from the earth in a state of high attenuation. Recently, the electricity occasioned by condensing steam has been exhibited on a very ex-

tensive scale, by the experiments of Messrs. Patterson and Armstrong, at Newcastle-upon-Tyne.*

From these considerations it would appear that all clouds at the time of their formation are electro-positive; although, in consequence of a slowness of the cloud-forming process they may occasionally give off to the neigbouring air the redundant electric fluid almost as rapidly as it is condensed. In such cases, the resultant cloud would be neutral; but when by a sudden depression of temperature a dense cloud becomes rapidly formed, its condensed electric fluid has no time to escape in any other manner than by a sudden discharge, which is a certain result if a proper object be sufficiently near to remove it.

The object nearest to a cloud thus rapidly formed, are other clouds, either previously formed, or just coming into existence: and as it is next to impossible that any two of these clouds should be in precisely the same electric state, the positive cloud of the group darts its lightnings to those around it which are less intensely charged than itself. These again, in their turn, discharge their lightnings to others *negative* to themselves: and thus it is that the greatest *quantity* of lightning is always among the clouds, the discharges to the ground being comparatively few. It is also a remarkable fact, that when lightning does happen to strike the ground, it is generally succeeded by a profound pause of comparatively long duration, during which not a glimmer of lightning is seen, nor is a murmur of thunder to be heard. Moreover, it is no unfrequent circumstance that the flash which strikes the ground terminates the electric storm.

The immensely large hailstones which frequently fall during an electric storm, even in the hottest part of the summer season, indicate a sudden depression of temperature to have taken place in the region of the clouds; and the subsequent cold that we almost invariably experience for several successive days, would lead us to infer that, whatever may be the cause of the depression of temperature, it originated at some considerable altitude in the atmosphere, and progressed downwards to the surface of the earth.

There is not, perhaps, a more prevalent idea respecting lightning, than that the danger is over immediately the rain commences falling. This is a sad mistake: and for want of knowing better many have become victims of this terrible element. Is there a summer passes over our heads without some fatal accident from lightning? Scarcely one. Men who are ignorant of the danger they are about to expose themselves to, and animals of all kinds, take shelter under trees, and other tall objects during an electric storm; not however from the lightning, but from the heavy rain which is falling. The tree is struck, and the ill-fated refugees

* A full description of these brilliant experiments appeared in the "Annals of Electricity, Magnetism, and Chemistry, &c.," vols. v, and vi.

killed on the spot, though prior to the fatal event no discharge had struck an object on the ground: every flash was between cloud and cloud, and the whole display in the aerial regions far above the loftiest object in the surrounding country.

That lightning strikes the ground more frequently during rain than previously is a fact that cannot be denied: and that this fact is strictly in accordance with the principles of electricity may easily be demonstrated both by analogy and experiment. An electric storm is generally preceeded by a period of dry weather, the atmosphere below the clouds being very dry, and, consequently a bad conductor, indeed it is ranked amongst the non-conductors, and the thickness of the stratum between the clouds and the ground is very considerable. But this is not the case in the region of the clouds; an immense quantity of aqueous vapour is there condensing, which renders the air a better conductor, and the distance between the clouds is but trifling. Hence, there is considerably less resistance between cloud and cloud than between the clouds and the ground; and though the difference of electric intensity might not be so great in the former, as in the latter case, the superior electric conduction, and the vicinity of the objects, tend to determine the discharges amongst themselves; and it is not till the falling rain has improved the conducting quality of the air below, and thus lessened the resistance to the electric force in the clouds, that lightning is capable of transpiercing it. It is therefore during the rain that the danger is greatest.

The experimental illustration of this topic is remarkably beautiful and satisfactory. For this purpose I employ a large Leyden jar, the universal discharger, and a plate of glass about a foot long. I bring the balls of the sliding wires of the universal discharger into contact with the glass plate whilst placed on the table, at about two inches distance from each other. One of the balls I connect with the metallic plate, and when the jar is charged to a high intensity I apply the discharging rod to connect the other with the ball of the jar: but no discharge takes place; which is in consequence of the distance between the balls on the glass being too great. I now bring the balls to about one inch from each other, get the jar up to the previous intensity, again apply the discharging rod and the discharge takes place. Now the resistance of a plate of dry air of about an inch in thickness, is nearly as much as the most intense charge of the jar is capable of overcoming. I will now moisten the air between the balls, by breathing on the glass, and I will remove the balls till they are three inches asunder. You will now see that the same extent of charge of the jar as before is capable of striking over the three inches of moist surface. I will now increase the distance between the ball to eight inches, and by means of a wet camel-hair pencil, draw an aqueous line between them. You will now have an opportunity of viewing a most beautiful phenomenon. The discharge of the jar traverses the eight inches between the balls on

the glass, and the fluid is seen in a compact body, with all the brilliancy of lightning, passing the whole length of the aqueous line.

The *striking distance*, in electric language, is any distance between two bodies through which the electric fluid is capable of passing, or striking, in a compact discharge; and as the striking distance is increased by an increase of intensity of the charge, and also by reducing the resisting character of the medium, it will depend upon both of these circumstances: that is, it will be directly as the intensity, and inversely as the resisting character of the aerial medium; which in symbols will stand thus: D is as $\frac{I}{R}$, in which D represents the striking distance, I the intensity of the charge, and R the resisting character of the aerial medium. Hence, when the intensity is constant, the striking distance will be reciprocally as the resisting medium; or, in still more general and familiar language, the striking distance is greater as the non-conducting quality of the air is diminished; and as the air has its non-conducting quality lessened by an admixture with water, the striking distance of lightning from a cloud in the direction of the earth must be greater during rain than when the air is not so charged with water.

Again: when the resisting medium is constantly the same, the striking distance will be as the intensity of the charge, or D is as I. Hence it is that in discharges of similar quantities of electric fluid from different sized jars, the striking distance between the ball of the discharging rod and the ball of the jar is very different, because of the difference of intensity in the two cases; the striking distance being always greatest with the smallest jar.

The subject of lightning conductors is a branch of practical electricity of exceedingly high interest, and demands the contemplation of the most profound electricians. Hitherto, however, little more has been attended to than the erection of a pointed rod of iron, without regard to situation, altitude, diameter, inferior termination, or any of those theoretical points essential to the efficacy and protection of the conductors, so as to render it a safeguard to persons and property against the most formidable element of nature.

Franklin, the inventor of lightning conductors, first proposed "for protecting houses, churches, ships, &c., from the stroke of lightning, to fix on the highest parts of these edifices upright rods of iron, made sharp as a needle, and gilt to prevent rusting; and from the foot of these rods, *a wire down the outside of the building into the ground, or down round one of the shrouds of a ship, and down her sides till it reaches the water*. Would not these pointed rods probably draw the electrical fire *silently* out of a cloud before it came near enough to strike, and thereby secure us from that most sudden and terrible mischief?"

This philosopher, however, subsequently recommended continuous iron rods, of about half or three quarters of an inch diameter; which he said "may be fastened to the wall, chimney, &c., with staples of iron. The lightning will not leave the rod, a good conductor, to

pass into the wall, a bad conductor, through the staples. It would rather, if any were in the wall, pass out of it into the rod, to get more readily by that conductor into the earth.

"If the building be very large and extensive, two or more rods may be placed at different parts, for greater security.

"Small ragged parts of clouds suspended in the air between the great body of clouds and the earth, often serve as partial conductors for the lightning, which proceeds from one of them to another, and by their help comes within the striking distance of the earth or a building. It therefore strikes through those conductors a building that would otherwise be out of the striking distance.

"Long sharp points communicating with the earth, and presented to such parts of clouds, drawing silently from them the fluid they are charged with, they are then attracted to the cloud, and may leave the distance so great as to be beyond the reach of striking.

"It is therefore that we elevate the upper end of the rod six or eight feet above the highest part of the building, tapering it gradually to a fine sharp point, which is gilt to prevent its rusting. Thus the pointed rod either prevents a stroke from the cloud, or if a stroke be made, conducts it to the earth with safety to the building.

"The lower end of the rod should enter the earth so deep as to come at the moist part, perhaps two or three feet; and if bent under the surface so as to go in a horizontal line six or eight feet from the wall, and then bent again downwards three or four feet, it will prevent damage to any of the stones of the foundation."

Such were the instructions of the celebrated Franklin; and had he recommended copper rods instead of iron, and directed them to be kept clear of the building instead of being fastened to the walls "with staples of iron," perhaps no better instructions could have been given; as far, at least, as an individual rod is concerned. But besides the injury that buildings may receive from a flash of lightning striking a conductor fixed close to the slates and masonry, from lateral explosions, a conductor consisting of a single branch only might be the means of drawing down destruction to some part of the building before the lightning reached the conductor; for, were the lightning cloud on one side of the building, and the conductor on the other, the lightning would neither go round nor over the house to arrive at the conductor, unless it met with greater resistance in a direct path, and as the destination of lightning is frequently a great distance from the cloud, and its path considerably oblique, it is possible that some part of its path might be through a part of the building before it arrived at a lightning rod which formed another part of its path.

Cases of this kind have occurred, and, consequently, may possibly occur again under similar circumstances: therefore it seems to me that unless lightning conductors of proper materials and dimensions, be properly placed, they may be the means of causing the most destructive consequences to those buildings they were intended

to protect. It is very seldom indeed that a flash of lightning proceeds in a vertical path: perhaps never.

I never yet saw, or heard of, a vertical discharge of lightning; they are frequently very oblique indeed. The lightning which damaged Saint Michael's Church, Liverpool, last year, was an oblique discharge, and struck the bronze cross at the top of the spire, several feet from its top.

There is such a display of ignorance in the erection of tall spires, that it is almost a miracle that the whole of them are not destroyed by lightning. The copper clamps and strings of lead, the former uniformly placed at intervals from each other, and the latter wantonly poured into the crevices of the masonry, render the spire a complete chain of alternate links of metal and masonry from top to bottom: the former inviting the lightning to the edifice, and the latter offering facilities for the most destructive explosions. From this very arrangement of the materials in the steeples of Saint Michael's and Saint Martin's, at Liverpool, and in the steeple of Brixton Church, have these three steeples been shattered by lightning. If such modes of building tall spires be indispensable to protect them from the power of the wind, conductors are quite as indispensable to protect them from lightning. Three copper rods at equal distances from one another, from the top of the spire to the ground, and united at the top, and by one or two bands below, would secure each spire from lightning on which ever side it approached.

Lightning rods, however numerous about a building, should have a general metallic union; they then form a system of conductors in which the force of the lightning would be divided, whichever branch was struck. I have a beautiful experiment to offer to your notice illustrative of this fact.

The apparatus represented by the accompanying figure consists of a series of iron-wire chains, so connected as to form a system of conductors of many branches. The chains hang vertically from a horizontal brass wire, and their lower ends rest on a sheet of tinfoil. The brass wire first receives the fluid from a discharge of the battery of jars, and the tinfoil carries it from the chains to the outside of the jar. The electric fluid, whilst traversing this circuit, illuminates every chain in the system to the same extent, showing that it is easily divided amongst them; and had there been ten thousand such channels it would have divided itself amongst the whole of them. This experiment shows two or more interesting facts. It proves that the iron scintillates at every link by an electric discharge through a chain of that metal; and these scintillations discover to us that the fluid occupies, and passes through, every channel in the circuit; and, as I shall prove more clearly by and bye, every metallic point in the chains throws off electric fluid into the air.

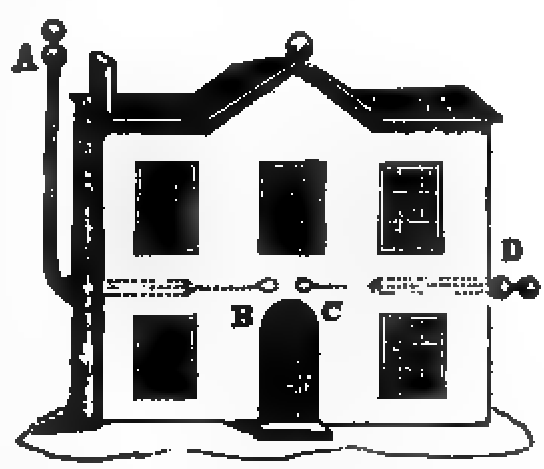
It is now time that we proceed with some of those popular experiments which have been established for the purpose of illustrating the beneficial effects of conductors when struck by lightning, and an experiment with the thunder house (an odd enough name) shall be the first on the list.

This long celebrated piece of apparatus is represented by the opposite figure, and consists of a model of the gable-end of a house, to which is attached a lightning rod, which can be made continuous or interrupted at pleasure, by means of a square piece of wood, which carries a portion of the rod, being placed in certain positions in a hole which it fits in the gable-end. When in one position its wire unites the other two portions of the lightning rod, but when in another position it disunites them. The lower extremity of the discharging rod is in connection with the outside of a Leyden jar, and over the upper extremity hangs a brass ball in connection with the inside of the jar. When the lightning rod is complete, and the machine turned till the jar charges sufficiently high to overcome the resistance between the two balls, a spontaneous discharge takes place, and the conductor protects the building; but if the square piece be placed in its hole so as to make a breach in the conductor, the next discharge of the jar throws it out of its place to some distance on the table, which is considered as a representation of a displacemnet of masonry in a building struck by lightning.

The electrical pyramid is another piece of apparatus for illustrating the efficacy of lightning rods, and of the danger to which such are exposed when so protected. The plinth on which the pyramid stands is fixed, and contains the important piece on which the fate of the edifice depends. The opposite figure is a representation of the apparatus in which the wire of the key-stone D is placed at right angles with the general direction of the lightning rod C C, and consequently there is an interruption at that place. The base of the pyramid is furnished with three balls, as feet, by which it rests on the plinth B F, one of the balls leaning on the moveable piece. Over the apex of the pyramid is sus-

pended a brass ball supported by a glass pillar. When the discharge of a jar is transmitted to the pendent ball, the lightning strikes that on the top of the pyramid, the brass rod of which conducts it safely to the ball at the bottom, but finding an interruption there in the metal, it explodes to arrive at the lower portion, C F, of the conductor, and blows out the key-stone which supported one side of the pyramid, when down it comes, and being made of several loose pieces which scatter about the table, its destruction seems complete. Had the wire of the key-stone been placed in a vertical position, it would have joined the other parts, C C, of the lightning rod, and the damage would have been prevented.

We have several other striking experiments for the purpose of illustrating the effects of lightning, but in all our models we are obliged to take advantage of good conductors, and give them such positions as may produce the intended effect. This figure represents the model of a house containing combustible materials, such as has already been shown will ignite by an electric discharge, and the result of the experiment with this model will afford a good idea of the probable consequence of a flash of lightning striking a building that contains inflammable articles.



This model is made of tin plate to prevent its entire destruction by the experiment. A glass tube passes through each of the two opposite sides of the model, which insulate two brass wires within them. These wires have each a brass ball at their inner extremities, on one of which is placed some tow moistened with oil of turpentine. The shorter wire C D, has a ring at its outer extremity, by which and a chain it is connected with the outside of a Leyden jar. The other wire, A B, is bent upwards, and the ball A at its upper extremity will receive the discharge from the inside of the jar. An explosion takes place within the building and ignites the tow and turpentine, producing all the appearance of a destructive fire.

The explosion of a powder magazine would be still more dreadful than the firing of a house containing other kinds of inflammable materials; and as the Royal Powder Magazines at Purfleet have been struck by lightning even when several lightning rods were attached to them, but fortunately without explosion of their contents, no means thought of for

their protection ought to be neglected. The model represented by the figure, will now receive a discharge from a Leyden jar, and the result will afford a pretty good idea of the effects of lightning should it enter a magazine of gunpowder. The electric fluid shall be discharged on the ball A, and conducted to the powder barrel at C, and the wire and chain D E B, will conduct it to the outside of the jar. A wet string is also in the circuit. The powder barrel explodes, blows the roof off the magazine, and levels the walls with the floor. The various parts of the model are joined together by hinges, and suffer but little from the explosion, so that the same model may be used several times.

Marine lightning conductors, or those employed on board of ships, are simply chains of copper, formed of links similar to those of the surveying chain, and are hoisted to the mast head when there is an appearance of lightning striking the ship. But the lightning has frequently struck ships before the chain could be got up: showing the propriety of having a permanently attached conductor, which would always be ready to receive and carry off the flash. Such fixed conductors have been proposed, and some are now on trial in the navy: but singular enough, these conductors instead of carrying the lightning overboard, would lead it into the body of the ship, and should they ever happen to be struck with a powerful stroke of lightning the consequences might be terrible indeed.

The idea of carrying a conductor through the body of the ship originated with Mr. Benjamin Cook, of Birmingham, about the year 1811, but it has been carried out by Mr. Harris, of Plymouth. Mr. H. has formed the conductors into strips of copper, which are inserted in grooves in the after side of the masts, from top to bottom, and through the keelson to the sea. In one of the smaller men of war, Mr. H. carried his mizzen conductor through the powder magazine!!! The evils attending these conductors, arise, principally, from *lateral explosions* and electro-magnetic influence. I have already illustrated the magnetic effects of electric discharges on a miniature scale, and from these we can form a good idea of the magnetizing influence of a flash of lightning passing through a conductor.

Imagine a chronometer to be placed near to a conductor carrying a heavy flash of lightning: the main and pendulum springs, the chain, arbours, and in fact every morsel of steel, would be rendered permanently magnetic, and consequently the machine rendered entirely useless: and the same fate would attend every chronometer and watch within the sphere of the electro-magnetic influence, which, in such cases, would be very extensive, and on every side of the conductor.

The lateral discharges are of three kinds, which I have distinguished 1st, 2nd, and 3rd, one of which I have already shown you, by the spreading abroad the grains of gunpowder, seeds, &c., and by the breaking of glass and other hard substances. These are the first kind, and take place at every interruption in the circuit.

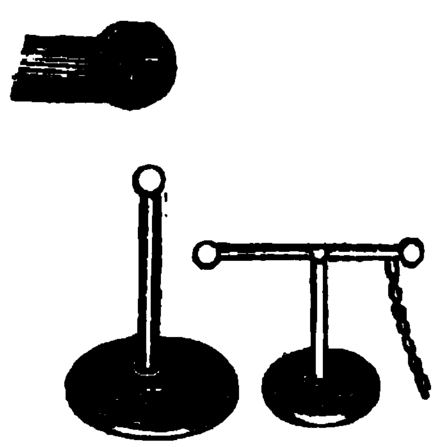
The second kind of lateral discharge, specimens of which I shall

now offer to your notice, occurs in the most perfect conducting circuit, unless the conductor be perfectly free from asperities, sharp angles, &c. I shall endeavour to illustrate this kind of lateral discharge by a few decisive experiments.

A thick copper wire, bent at various places into angles is suspended in the room, and I transmit a discharge of the battery, from a high intensity, through this wire. The room being darkened, the discharge takes place; and you will have observed that at every angle in the wire a brush of electric light sprang into the air.

I will now make a new circuit for the next discharge of the battery to traverse. In this circuit I place a strip of sheet copper about a foot long, in imitation of the copper conductors in masts. On making the discharge, both edges of the copper, from top to bottom, throw out fringes of electric fluid; which is a clear proof that in every discharge of lightning on to such conductors, an immense portion would issue from their edges, from the highest point of the mast to the step in the hold of the vessel. Of the consequences of such lateral discharges in the hold of a ship I must leave others to judge.

The third kind of lateral discharge takes place even from the best polished conductors under certain circumstances, and arises from the polarizing influence of the electric fluid whilst in motion. I cannot illustrate this kind of lateral discharge better than by two metallic rods, one of which shall receive miniature flashes of lightning from the prime conductor, and the other shall be placed near to the former. To insure the best conducting channel for the rod which receives the fluid from the prime conductor, I connect it by copper wire with the rubber of the machine, and also with the gas pipes which lead to the gas-works. The other brass rod is in connection with the table, but which has not the opportunity of carrying away the fluid as the other rod possesses.



The arrangement is represented by the accompanying figure, in which the vertical rod receives the sparks from the prime conductor, and simultaneous lateral sparks are seen between the vertical rod and the vicinal end of the other. If, instead of sparks from the prime conductor, I discharge a jar down the vertical rod, the lateral discharge is seen as before. If I insulate the horizontal rod, and place another near to its remote extremity, lateral discharges take place between these two simultaneously with the other. These effects are truly in miniature, but beautifully illustrative of those which happen from the discharge of lightning on a conductor situated near to other conducting bodies. But it is not requisite to watch a lightning conductor till it is struck by the primitive discharge from a cloud to convince us of the danger attending this class of lateral explosions; since an atmospheric electric wave, produced either by a distant flash of lightning, or by the transit of

a highly charged cloud, not only satisfies the curiosity on this point, but demonstrates the fact in the most ample manner. Many are the instances of this that I have witnessed whilst experimenting with an elevated kite.

For the purpose of contemplating lateral discharges on such occasions, I insert a stout brass rod deep in the ground, and bend its upper part so as to lean towards a reservoir, which receives dense sparks, and corresponding lateral discharges take place between that lightning rod and other vicinal conducting bodies, as in the experiment already passed through.

But the most splendid series of experiments in illustration of electric waves, and of the lateral explosion at the same time, were made by Mr. Weekes, of Sandwich, on the 19th of May, 1841. This indefatigable philosopher has a stout copper wire suspended between the steeples of two churches, over a part of the town of Sandwich, and from this wire another descends to his laboratory, and ready to be attached to any piece of apparatus with which he is about to make experiments. In compliance with the rules which I have shown you for illustrating this kind of lateral explosion, Mr. Weekes arrayed his apparatus in the most suitable manner for obtaining brilliant results: which were most amply displayed on the 19th of May last.

A black cloud passed over the town, and flashes of lightning were seen, but no direct discharge ever touched the apparatus; nevertheless, " a mighty torrent of dense sparks, so vivid as to dazzle the eye of the observer, attended by contemporaneous stunning reports, and fraught with an unusual intensity, rushed from the terminus to the ball in communication with the earth," though separated to a distance of three and three quarter inches; at the same identical moment " a furious current of *lateral sparks* takes place between the wire and leaden spout of the pump." Such is Mr. Weekes's own description of the first part of the electrical drama.

" But now comes to be described the most resplendent feature of the scene before us; the iron nail, serving to connect the pump machinery, suddenly exhibits the appearance of a magnificent firework, the splendour of which is repeatedly enhanced as *waves* of electric fluid rush through the arrangement, in obedience to each successive lightning flash from the storm cloud, and this sublime scene, with short intervals of lesser energy in the electric current, continues through the space of one hour and sixteen minutes. The combustion of the iron nail forcibly reminds me of the appearance which that metal exhibits when burnt in oxygen gas, or rather when brought under the influence of the oxy-hydrogen blow-pipe, though the phenomenon was accompanied by a deep red kind of light, which does not belong to either of these comparisons."*

The iron nail which deflagrated so splendidly was absolutely in the circuit, and that circuit completely metallic down to the bottom

* Annals of Electricity, vol. vi, p. 450.

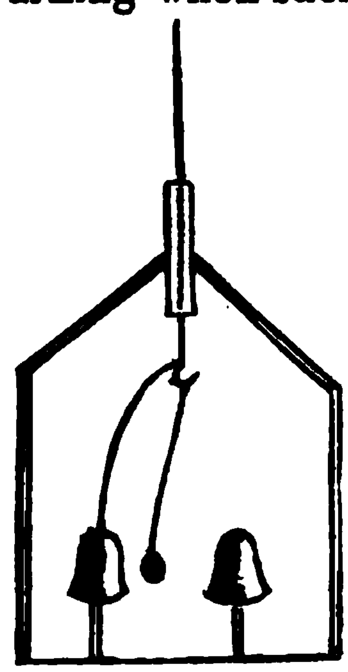
of the pump pipe in the well of water; the phenomenon was, therefore, a lateral discharge of the same kind as that exhibited by the bent wire and strip of sheet copper. The other lateral discharges which Mr. Weekes observed were of the third kind. These he took from every part of the wire, pump, and other parts of the conducting circuit, many of which were productive of powerful shocks. A young lady, who accidentally stepped on the wire, received a lateral discharge which sent her " reeling across the laboratory."

Such facts as were developed in this series of experiments, and many others on record, and especially those of M. de Romas, made with an electric kite on the 7th of June, 1753, in which immense torrents of the electric fluid streamed down the string though never struck by lightning,* are not only necessary to be borne in mind by every kite experimenter, but should ever be kept in view by projectors of lightning conductors.

The electro-magnetic action of lightning is also an essential consideration, especially on board of ship; and if it were on no other account, this alone is of sufficient importance to discourage the idea of carrying a lightning conductor through the body of the vessel.

Marine lightning conductors should always be so placed as to carry the lightning over-board, and not so as to entice it into the vessel. Hence it is that even the usual chain conductors, if got to the mast head in time, are a greater protection to the vessel than a conductor that would lead the lightning into the hold.

When Franklin had discovered that tall pointed rods would draw the electric fluid from the air during the transit of an electric cloud, he contrived an ingenious apparatus to give him warning when such clouds were passing over his dwelling. The indication given by this apparatus was a ringing of bells, upon the principles shown in a former lecture.



The cut represents Franklin's atmospherical electric bell apparatus. The outside frame represents a section of the roof and walls of the house, from the top of which rises a pointed rod, which is insulated by passing through a glass tube in the roof. On the floor are placed two small bells, one of which is supported by a glass pillar and the other by a metal one. A wire connects the lightning rod with the insulated bell, and a small metallic ball, suspended by a silken thread, rings the bells when the insulated rod and bell become electrized by a *wave* which a passing cloud produces.

If we place this model at the distance of a foot from the prime conductor, and put the machine into motion, you will find that the bells begin ringing by the fluid drawn from the air by the pointed

* Annals of Electricity, vol. v, p. 63.

rod; which gives a good idea of the indications afforded by Franklin's apparatus when an electric cloud passed over his house.

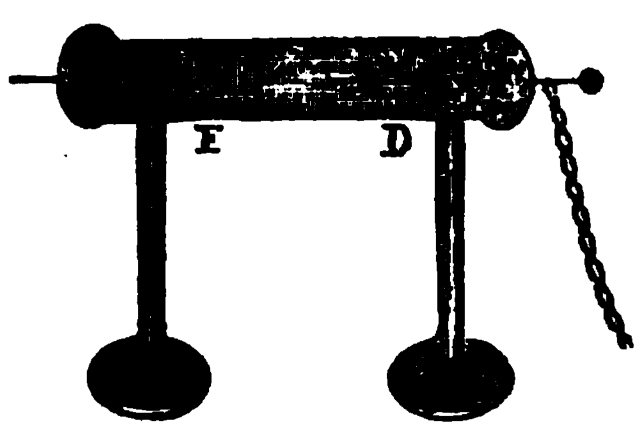
When lightning happens at a great distance from the observer, its effects are seen in the horizon amongst the clouds and vapour that are hovering in the atmosphere; it is then called ***sheet lightning,*** and by many persons considered to be of a different kind to that in which the electric fluid is absolutely seen darting through its zigzag path, and attended with loud thunder. This prevalent error may easily be illustrated by a very beautiful experiment which I will offer to your notice.

I place on the electrical stool three pieces of tinfoil in the same right line, leaving an opening between the ends of the middle strip and those of the other two. Over each opening I place a large decanter about half filled with water, to represent the clouds illuminated by the discharge of lightning. Having charged a large jar, and connected its outside with one of the outer strips of tinfoil, I apply the discharging rod to the other extreme strip and the ball of the jar. The discharge takes place through the intervals beneath the decanters, and these vessels, with their contents, are highly illuminated. In this experiment I take the precaution to screen the electric fluid from the spectators, so that nothing but its effects are seen in the decanters and water. This is an experiment beautifully illustrative of sheet lightning, which is merely the effect of a distant storm.

LECTURE XX.

In a former lecture, I have stated that the resistance of atmospheric air is much abated, when highly attenuated, and now I will solicit your attention to a few illustrations of that fact, and to some interesting phenomena which will attend them.

The instrument represented by the figure is called the *luminous conductor,* because of its beautifully illuminated interior during its electrization. This apparatus consists of a glass cylinder, about three feet in length and five or six inches in diameter, terminated at both extremities with hollow brass hemispheres: a point at one end for collecting the electric fluid, and a ball at the other end, from which sparks are taken.



The hemispheres screw on to brass caps, which receive, and are cemented air tight upon the ends of the glass cylinder. A metallic point projects inside from the centre of one cap, and a ball projects inside from the centre of the other; so that when the hemispheres are properly placed, as in the figure, two points are connected to one of them, and two balls to the other. When the hemisphere carrying

sudden connection between the ball on the top and the inside of the the ball is removed the cap beneath exposes a stout brass pipe, tapped and furnished with a valve, for the purpose of being screwed to an air pump, and the air within the glass cylinder attenuated to the highest possible degree: which done, the hemisphere is replaced, and the apparatus, thus prepared for experiment, is laid on the two crutches which surmount the glass pillars, as seen in the figure.

If we now present the point of the luminous conductor to the prime conductor, it draws off the fluid and conveys it to the attenuated air inside the glass; and the air being now a tolerable good conductor conveys it onward to the remote brass cap, from the ball of which it may be received in sparks or otherwise as decidedly as from the prime conductor itself. But the beauty of experiments with this apparatus consists in the variegated light which fills the glass cylinder, and shows at first view, that the electric fluid expands when permitted, and occupies every part of the conducting medium. When the remote extremity of the luminous conductor is connected with the floor we behold a steady purple-tinged cylinder of the electric fluid; but if the fluid be taken away in sparks, a momentary darkness succeeds each, causing the light within the conductor to quiver in correspondence with the sparks. A similar agitation of the light is occasioned by removing the exterior metal point, and permitting sparks to pass between the two conductors. Experiments with the luminous conductor are amongst the most interesting in electricity, especially when exhibited in a well darkened room, and with a powerful machine in action.

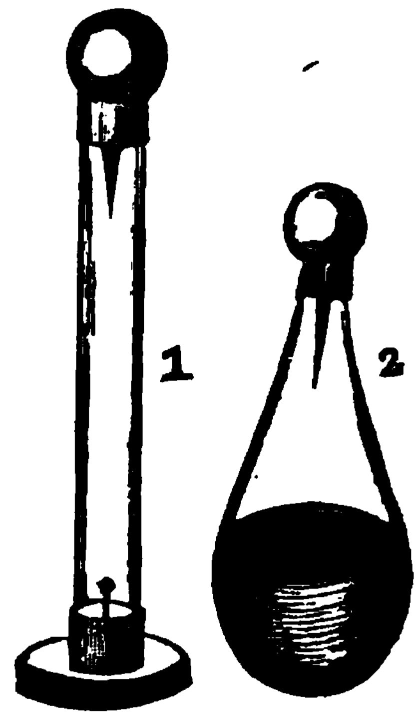
When the air within a vessel is not too much attenuated, the electric fluid is divided into a cloud of quivering streamlets, intersecting one another in a capricious and most astonishing manner, producing an ever varying fantastic reticulation, &c.

The beautiful variegations exhibited within the luminous conductor are usually resorted to as illustrative of the electric origin of the *aurora borealis*, or northern lights. This natural phenomenon does certainly, on some occasions, put on precisely the same appearance as that seen within the luminous conductor, and its being displayed in the higher regions of the atmosphere, where the air is much attenuated, is strictly analogous to the conditions of the experiment; and although there are frequent displays of auroral phenomena which have not yet been imitated by electrical experiments, there can be little doubt of the whole of them emanating from an electric source.

In the space intervening the region of the aurora borealis and that of lightning, is another beautiful electrical meteor called the falling star. This phenomenon we also imitate by a beautiful electric experiment. For this purpose I employ a tall glass tube, represented by No. 1, in the following figure. It is furnished with balls, points, and a valve in precisely the same manner as the luminous conductor, but the air within the glass tube is not so highly rarified. I now charge the battery to the highest intensity that I think it will stand; and having the lower brass cap of the apparatus in good metallic connection with the outside coating, I make a

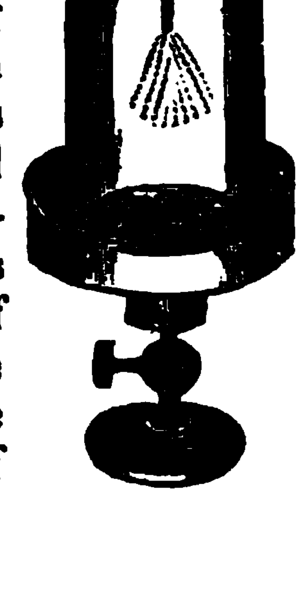
battery, and the discharge takes place through the four feet of attenuated air, the fluid traversing it in a compact mass highly imitative of the meteoric star. When the room is darkened this is an exceedingly beautiful experiment, and gives an opportunity for the eye to follow the electric ball from the top to the bottom of the tube.

Since attenuated air is a good conductor, it may be employed as a coating to glass, in the place of tinfoil. This fact was first shown by the Abbé Nollet, who charged a bottle without any metallic lining, and on discharging it with his hand received a more violent shock than he had been led to expect. The instrument now employed to showth is fact is a Florence flask, having its neck enclosed in a perforated brass cap, furnished with a valve, and tapped for the application of the air pump. A pointed wire projects inwards from the metal cap, and a hollow sperical ball terminates the cap exteriorly. A small portion of the bulb of the flask is covered with tinfoil, but none within. A representation of this apparatus is seen as No. 2, in the cut.

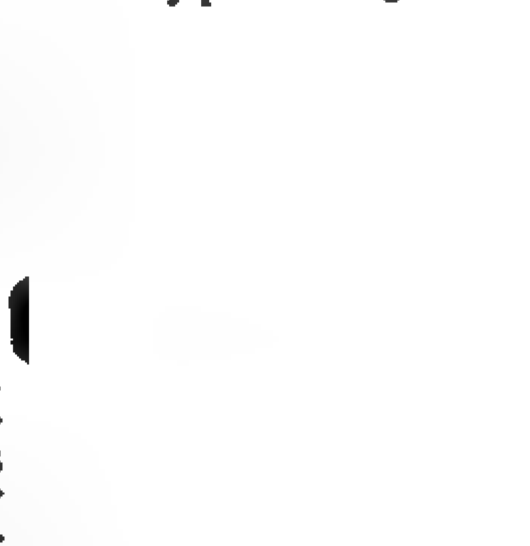


On presenting the ball of the exhausted flask to the prime conductor, whilst holding the coated part of the glass in the hand, a beautiful purple brush of light is seen to issue from the metal point, and spread itself over that part of the interior of the glass whose outer surface is coated with metal. If the charge gets two high for the flask to retain, the fluid will either flow over the top, from the edge of the cap to the metal coating, or it will spring through the solid glass, which it perforates, and thus renders the apparatus useless. If when the charge is high the hand be brought to the ball, a shock, even more violent than from an ordinary jar of the same magnitude, will be experienced. A residuary charge however is still left behind, which requires many contacts to dismiss entirely; and as each partial discharge is attended with a display of feeble purple light, the flask exhibits a series of beautiful flushes in a darkened room for a long time after the first discharge.

If, instead of the partially coated flask used in the last experiments, we were to employ a glass receiver, on the transfer plate of an air pump, as represented in the figure, you will see the electric fluid shooting downwards through the attenuated air in a beautiful divergent brush of light, from the wire that passes through the cover to the metallic pump-plate: and in this case the fluid is conveyed away without charging the glass. But if I touch the side of the glass with my fingers, they become so many patches of coating, and the electric fluid bends towards them in the most fanciful manner, and charges the opposite surface of the glass. By changing the position of



my fingers I can lead the electric fluid to any part of the glass that I please. This is a beautiful experiment when the room is darkened, and is strictly conformable to the doctrine of the Leyden jar, already illustrated. The opposite figure will give some idea of the bending of the fluid towards the fingers.



By varying the density of the air in long glass tubes, we find that its resistance to electric transmission increases with its density; and consequently becomes a better conductor in proportion to its rarity: the probability therefore is, that vacuous space offers no resistance whatever, and thus becomes the best of all conductors.

Since we have shown, in a former lecture, that the conducting metals constituting the coatings of a Leyden jar, retain but a small fraction of the charge, what are their uses? is but a natural question. They are indispensable both in the charging and discharging process. In the former process, one of the coatings receives the fluid at a mere point only, but by its conducting character it is enabled to distribute every spark over the glass surface which it covers; whilst the opposite coating allows of the departure of the fluid from the other surface. In the discharging process they perform the reverse functions, and allow of a sudden discharge from the electro-positive to the electro-negative surfaces. Independently of these appendages, the charge could never be equally distributed, nor could a discharge be sudden and complete.

The doctrine of electric atmospheres is a subject of great interest, and is interwoven with the display of every electrical phenomena. It is a subject which requires much force of reasoning for its clear and satisfactory demonstration, and extensive series of experiments for its complete illustration. We must, however, on this occasion, content ourselves with a brief illustration of this beautiful doctrine.

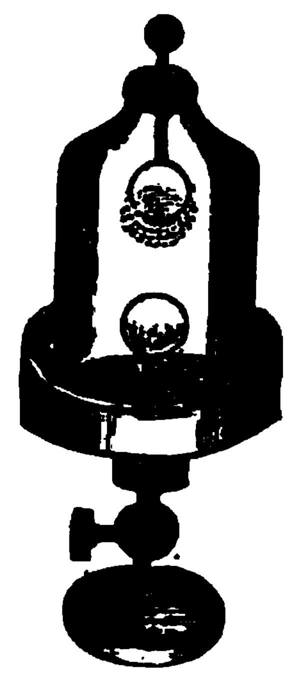
It appears that Otto Guericke, Burgomaster of Magdeburg, about the year 1670, was the first philosopher who noticed electric atmospheres, and their effects on bodies immersed in them: but the late Lord Stanhope, about 1778, seems to have studied the doctrine to a much greater extent. It is supposed by this nobleman that all bodies in an electric condition electrize the air around them to a considerable extent, and this electrized air is the atmosphere in question. Stanhope's two grand propositions in this doctrine are as follows.

"If a body be *positive*, and if it be surrounded by *air*, that electrified body will deposit upon all the particles of that air which shall come successively into contact with it a proportional part of its *superabundant* electricity, by which means the *air* surrounding that

body will become *positively* electrified: that is to say, it will form around that *positive* body an electrical *atmosphere* which will likewise be positive.

"If on the contrary, the body be *negative*, each particle of air that shall come into contact with it will deposit thereon a certain part of its *natural* share of electricity, by which means the circumambient *air* will become *negative*: that is to say, it will form a *negative atmosphere* around the body which is *negatively* electrified."

Beccaria, the famous Italian electrician, who perhaps studied the doctrine of electric atmospheres with greater care than any other philosopher, instituted a most beautiful experiment, by means of which an electric atmosphere is rendered perfectly visible.



I will endeavour to repeat this grand doctrinal experiment by means of the apparatus represented by the figure. The apparatus consists of a glass receiver with a brass cap and an air-tight sliding wire; and a transfer plate of an air pump. The sliding wire is furnished with a ball at each end; and another short wire which rises from the pump plate is also surmounted by a brass ball. Having attenuated the air within the receiver, I remove it from the pump, screw on to the lower end of the pipe its wooden foot, and place the whole on an insulating stand. I now connect the upper wire with the prime conductor and uninsulate the transfer plate with its ascending stem and ball. The machine being in good order is now to be brought into play. No sparks are allowed to play between the two balls in the receiver, but their polarization is perfect and complete: and the accumulated fluid on the lower side of the upper ball, is distinctly seen as a luminous electric atmosphere, covering about half of the ball. This phenomenon is represented in the figure by the dotted atmosphere round the lower half of the ball.

I will now invert the order of arrangement; by insulating the lower ball and connecting it with the prime conductor, and the upper ball of the apparatus I touch with my finger. Under these circumstances the luminous atmosphere appears on the upper side of the lower ball, and none on the upper ball; and by reversing the arrangements a few times we discover that the ball alone, which is connected with the prime conductor, displays the luminous electric atmosphere.

If now, whilst the lower ball is in connection with the prime conductor, and the machine in action, I press down the sliding wire gradually, the luminous atmosphere on the lower ball expands upwards, gradually forms into a round-topped cone, and at last discharges itself in a dense spark to the upper ball.*

* This last variation of the experiment, I believe is quite novel. I have also shown that the luminous electro-sphere can be produced independently of attenuated air. See Annals of Electricity, vol. ii, p. 413.

What a fund of intelligence is opened to our view by the display of these beautiful phenomena! An electric atmosphere is here exposed to our view, and no longer rests in the imagination alone; and as this phenomenon appears at the positive ball alone, it is one of those principal supports of the doctrine of a single electric fluid. It proves, also, that the electric fluid is *self luminous;* and the last phenomena exhibited by the experiment show that polarization precedes discharge.

We have no experiment, that I am aware of, that would favour the theoretical views of Lord Stanhope on this subject, unless under circumstances in which the charge of a body could be thrown off into the atmosphere: or, according to his second proposition, where the electric fluid could enter the negative body. When the bodies are rounded and well polished, they neither *receive* nor *deliver* the fluid easily from, and to, the atmospheric air: and the polarization of such bodies must be very high before a discharge could be accomplished from one to the other through a thick plate of dense air, and especially when the vicinal surfaces are but little convex. Hence it is, that in the electro-polarizations already shown in an early lecture, no discharge took place from the positive disturbing body to the vicinal negative surface of the polarized body, except in those cases where pointed wires were employed and an uniform current transmitted.

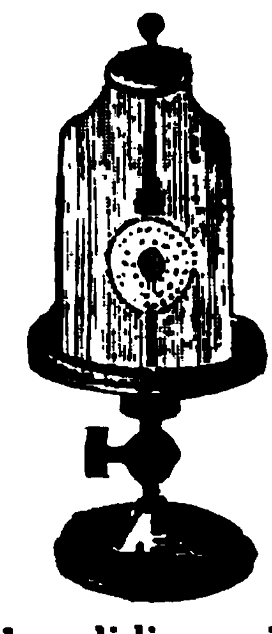
The luminous *electro-sphere* of Beccaria, affords no idea of the electric matter being thrown into the surrounding air; but, on the contrary, would lead to the belief that the accumulated electric fluid *repels* the air with its contained electric particles towards the opposite ball, and not being able to enter its smooth surface renders its vicinal side negative by a *secondary* polarization: the polarization of the intermediate air being the *primary.* This view is supported by the fact that when the polarized body has points or sharp edges at its remote side from the polarizing body, that its own electric fluid can be *driven out* of it by the repulsive action on the opposite side: but no fluid enters from the neighbouring air to make up the deficiency; consequently we find the body negative when the disturbing positive body is withdrawn.

Beccaria, the illustrious Italian philosopher who discovered the luminous electrosphere shown in the last experiment, also devised another experiment, by means of which I shall be enabled to convince you of the resistance which the electric fluid meets with on its approach to smooth convex or flat metallic surfaces.

The apparatus for illustrating this interesting fact, is that used in the last experiment, with the addition of a Leyden jar. The air in the receiver being attenuated as before, and the jar charged to a low degree of intensity, I discharge it through the receiver, and you will observe a narrow cylinder of light between the two balls, which spreads over the upper surface of the lower ball for a perceptible time before it disappears. The jar is next charged to a little higher intensity than before, and when discharged, the cylinder of light is of greater dimensions than by the first discharge; and a much greater

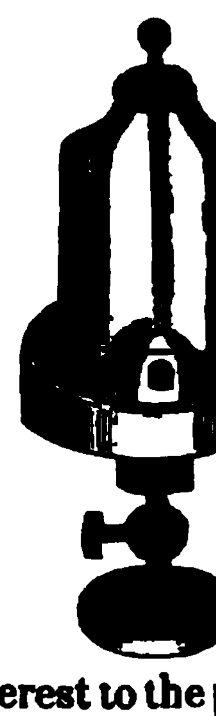
portion of the lower ball is covered with the electric light than before.

To enhance the beauty of the experiment, I will next use three of the battery jars. When these are charged pretty high, I transmit their contents through the receiver, and now, instead of a partial covering of the lower ball, the whole of the surface and that of its stem of support, are completely enveloped in a luminous electrical cloud of some duration, which seems to find more difficulty in entering the polished surface of the ball, than flowing over it through the attenuated circumambient medium to the asperous surface of the pump plate which it enters, and thus disappears. The apparatus with the enveloping cloud on the lower ball, is represented by the opposite figure.



If, instead of a Leyden jar, we were to connect the sliding wire with the prime conductor, the stream of electric fluid between the two balls, and the luminous cloud on the top of the lower ball, might be continued for any length of time we pleased; but the light is but faint, and can only be seen by close observers.

Another experiment established by Beccaria, shows the direction of the fluid through the attenuated air, and forms a beautiful cascade. On the lower plate is placed a hollow hemisphere of glass, as represented by the accompanying figure, and the ball is removed from the lower end of the sliding rod, which is again connected with the prime conductor. On bringing the machine into action, a stream of light flows from the lower extremity of the sliding wire and falls on the top of the hemisphere, which it partially illuminates; and in a short time the stream trickles over one side of the hemisphere to the pump plate as shown in the figure. The *direction* of the electric stream is decisively shown and well defined, which gives a pecular interest to the phenomenon. The cascade, however, does not continue constantly on the same side of the hemisphere, but removes from place to place, which gives it a more lively and pleasing appearance.



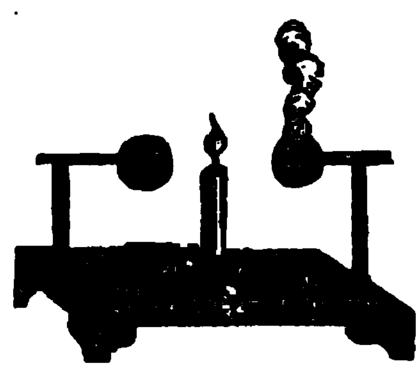
In the preceding experiments the air within the receiver was not highly attenuated, nor the machine in full action; but if the attenuation be carried on till the air pump ceases to act on the remaining air, and the machine be brought into full play, a stream of purple electric light falls upon the surface of the lower ball, which it does not enter but breaks upon it, and runs over it and its stem to the pump plate in a beautiful cascade.

The *direction* of the electric discharge is demonstrated by several other experiments, one or two of which I will now proceed with. I place two sticks of sealing-wax close together, laterally, on the table of the universal discharger, so as to form a channel at the juncture of their rounded edges: on this channel I place a cork ball of about an inch in diameter. When the balls of the sliding wires are re-

moved, I direct the points towards the ball, each being about three inches from it and pointing towards its centre. One of the sliding wires is connected with the table, and the other with the prime conductor. On turning the machine gently the ballrolls along the groove *from* the positive to the negative wire.

I now remove the sealing-wax and the cork ball, and place on the table of the universal discharger a lighted candle, the flame of which is about the height of the sliding wires when placed horizontally. The points of these wires are directed towards each other, having the flame of the candle directly between them. The machine is put into motion, and the flame yields to the positive aura and bends towards the negative wire, now in connection with the rubbers of the machine. If the flame of the candle be blown out, the smoke from the wick still bends in the same direction.

Both the prime and the negative conductors being insulated, I place the apparatus represented by the opposite figure in connection with them by means of wires, one to each of the insulated horizontal wires. Each of these wires carries a metallic hemispherical cup, in which is placed a small piece of phosphorus, and between is a burning taper. I now put the machine into motion, and in a short time you will observe the phosphorus in the negative cup inflame, but the other piece does not.

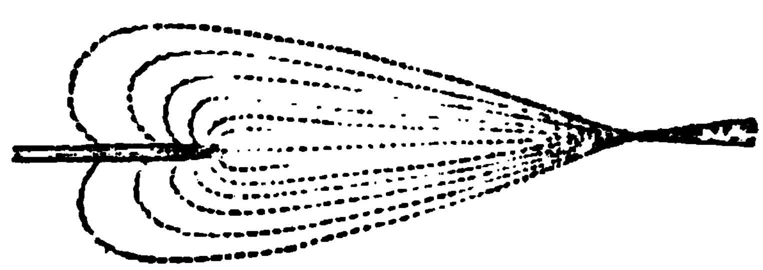


These are some of the experimental data which have been brought forward in favour of the doctrine of *one* electric fluid only. There are several other phenomena which tend to give support to that doctrine, but having selected those which appear most satisfactory, it would be needless to dwell longer on this part of our subject. It may be necessary, however, to observe that, although an electric current proceeded from the point of the positive wire when operating on the cork ball, flame, smoke, &c., there can be no doubt of the existence of a current of air also, which added to the mechanical action.

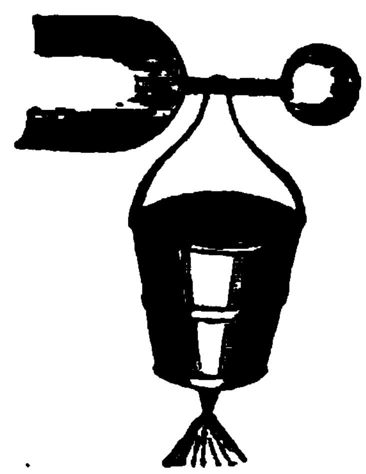
When the back part of the hand is presented to the point which throws out the aura, a gentle cool blast is experienced; and uninsulated bodies, although attracted by presenting them to the *side* of the wire, are absolutely driven away from the point of it. If, for instance, I suspend a light pith ball by a moistened hempen thread held between my finger and thumb, and present it to any part of the prime conductor, or to the side of the pointed wire fixed into its remote end, the ball is forcibly drawn into close contact with the metal, where it will remain as long as the machine keeps in action; but if I present the ball to the projecting point of the wire it is driven off, and will not come near to it.

The pith ball, in this case, may represent a particle of air, which being first attracted to the side of the wire, would travel towards the point, in consequence of the electric force being gradually stronger in that direction; but when it arrived at the point itself, it would be thrown off by a repulsive force, and multitudes of particles of air following its example, would produce a current of air from the point.

It appears singular, at first sight, to observe negative points which are *receiving* electric fluid from the air, repel the uninsulated ball as decidedly as those positive points which throw the fluid off; but the immediate cause is the same in both cases, for it can be shown that a current of air proceeds from the negative point also. The air contiguous to the side of the wire is attracted more and more towards the negative point, where it deposits its electric fluid, and afterwards driven off by succeeding portions, which in their turns are driven off also; hence a continual wind is kept blowing from the negative point. The opposite figure will give some idea of the manner in which the electric fluid would rush out of a positive point through the air to the negative wire.

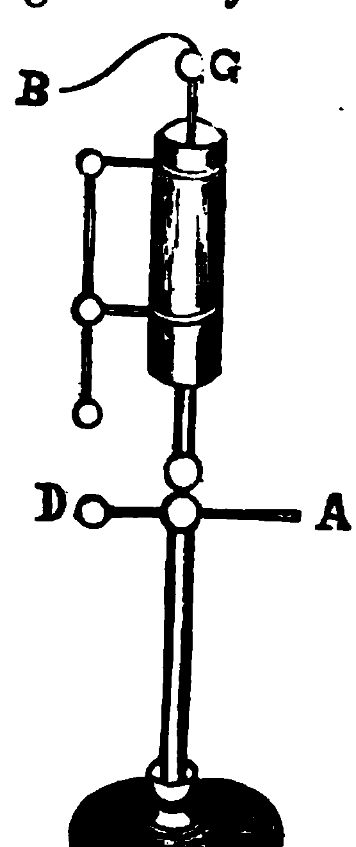


If instead of a metallic point we were to employ water within a capillary tube, that water would be thrown out in a divergent stream similar to the aura in the air. For this purpose we employ a small metal bucket with a capillary tube inserted in the bottom. When this bucket is partly filled with water, small drops occasionally fall from the lower orifice of the tube; but if it be hung on the prime conductor, as represented by the cut, and the machine in action, the water flows copiously in a divergent stream, as represented in the figure. When the room is darkened the divergent current is slightly luminous.



In a previous lecture I promised to bring forward some of those contrivances which have been invented for the purpose of measuring the *quantity* of electric fluid constituting the charge of Leyden jars.

The only instrument for this purpose that has gained any celebrity amongst *writers* on electricity, is called the *unit jar:* and as we have one of these unit jars belonging to the Institution, I will describe its structure and point out its principles of action with some degree of minuteness. It is represented opposite.



This celebrated instrument is formed of a small cylindrical Leyden jar, supported in an inverted position on a glass stem, well covered with lac-varnish, and fixed into a wooden foot, as seen in the figure. The inner coating of the jar is in metallic contact with a brass ball and wire D, A; another ball above the former is in contact with the outer coating of the jar, by means of a metallic frame and sliding wire. The brass ball G, and wire B, are also connected with the outer coating. The arrangement of this apparatus, is obviously the same as that of the medical jar, with an at-

tached Lane's discharger, for if the wire A D be connected with the prime conductor, and the wire B with the ground, the charge will proceed till the resistance between the ball D, and that above it is overcome; which accomplished, the jar will discharge spontaneously; and so long as this resistance is constant, and the outside surface uninsulated, similar quantities of fluid will cause corresponding discharges. Now as the spontaneous discharges take place between the two fixed balls, the striking distance is constant, and the discharge through that striking distance will depend upon the *intensity*, and not upon the *quantity* of fluid in the unit jar. For convenience we will call this requisite *intensity* the *discharging intensity;* which would be constantly the same if the striking distance presented a constant resistance; although the *quantity* of fluid required for this *discharging intensity* might vary considerably, according to the facility afforded for displacement of the fluid from its outer surface.

If, for instance, we have two jars, whose figure and extent of coated surfaces were precisely the same, but the thickness of the glass considerably different, that made of the thinner glass would require much more fluid than the thick one, to arrive at any given intensity of charge; because of the latter offering a greater resistance to the disturbing force of the accumulated fluid within; and if the resistance were augmented by any means whatever, the standard intensity would be arrived at by a still *less quantity* of electric fluid. Such are the considerations to be attended to in explaining the operations of the unit jar.

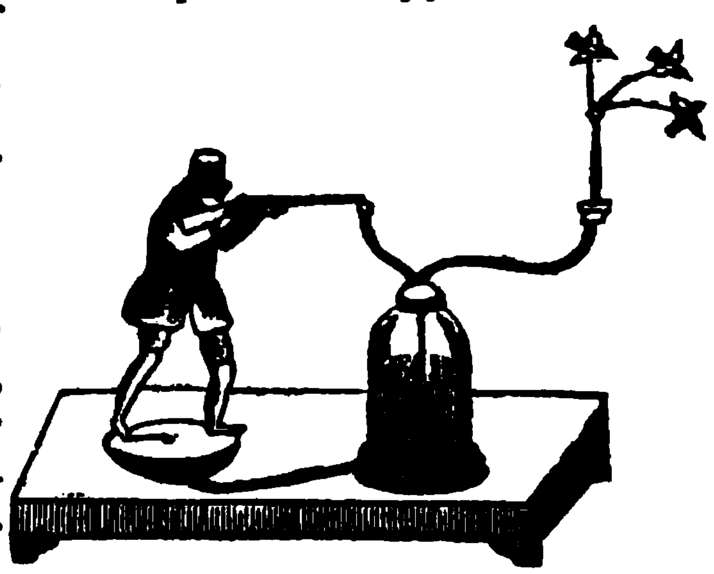
When this instrument is used with a view of measuring the quantity of electric fluid which charges another jar, in all cases much larger than itself, and which for convenience we will call J, the wire G B is connected with its inside, and the wire A D with the prime conductor. When the machine is put into motion, the *unit jar* charges; a portion of the fluid belonging to its outside being driven into the inner surface of J, which, consequently, to a certain extent charges also by polarization. Now the quantity of fluid driven into the unit jar will depend upon the quantity driven into the jar J, and the quantity driven into C, will depend on the thinness of its glass; therefore, the first unit of fluid for the *discharging intensity* depends upon the substance of glass of the jar J, and however thin that glass may be, the *discharging intensity* will require less fluid in the unit jar than when its coating was connected with the ground.

We now suppose that the first discharge has taken place, and that *nearly* an unit of fluid is thus thrown into J (the *whole* could not be thrown in because of the *initial* part of the discharge partially charging the outer surface in common with the inside of J). The resistance of J against the reception of fluid from the outside of the unit jar is now increased, and the *discharging intensity* will be accomplished by a less quantity of fluid than at first: and this second discharge of the unit jar throws a still *less proportion* of the *diminished* quantity into J than in the previous discharge. And

thus it is that each succeeding charge requires less and less fluid for the *discharging intensity,* and a corresponding disproportion enters the jar J.

When the intensity of J becomes considerable, the unit jar will be nearly choaked up, and incapable of receiving any but a very trifling quantity of fluid; and were the resistance of the striking distance not altered during this time, the discharges from the ball D to that above it would take place as if no unit jar were there. This resistance, however, is increased, which requires a higher *discharging intensity,* and consequently somewhat more fluid than if no increase of resistance had taken place. But this increase of resistance in the striking distance is that which lessens the *quantity* discharged, which at high intensities of the jar J is very small indeed.

The electrical sportsman is another piece of apparatus which operates upon the principles of Lane's discharging electrometer. The opposite figure represents this apparatus, which consists of a Leyden jar, a flock of birds, and the sportsman with his gun. From the inside of the jar proceeds a long bent brass wire, with a small wooden stage near its remote end. To the extremity of the wire four or five threads are tied having artificial birds at their other ends, which rest on the stage. Another wire, terminated with a small ball, also rises a short height from the inside of the jar. The outside of the jar is connected with the table and also with the gun, at the end of which is a small ball, and brought to within striking distance of the ball of the jar.



The inside of the jar being connected with the prime conductor, and the machine put into motion, the charge proceeds, and at the same time the birds rise and fly from one another by repulsion, until the *striking intensity* discharges the jar to the muzzle of the piece, when a flash is seen, and the birds drop as if shot by the discharge.

Whilst on the subject of electrized glass, I will offer to your notice a few curious experiments on flat glass plates. I will first operate with the glass discs, and the two metal discs before alluded to. I place the glass plates between the two metallic plates, and charge the upper side positively by uniting it with the prime conductor whilst the lower surface is uninsulated. This done I remove the connections with the prime conductor and the table, and then discharge the glass plate by an application of the discharging rod. When the discharging rod has been removed I take up the upper metal plate by its glass handle, and by applying it to my knuckle I receive a feeble spark. I now replace this plate on the glass, and with one hand I touch the other plate: on approaching the upper plate with the other hand I again experience a spark. I

now lift up the upper plate by its glass handle and experience another spark on presenting the knuckle, in precisely the same manner as with the electrophorus. But what is very remarkable, the power of these sparks increases to a great extent, and again diminishes, and so on for a long time together.

I now employ two square glass plates, each of which is coated on one side only; I place the naked surfaces upon one another, and press them close together. This done, one of the coatings is connected with the prime conductor, and the other with the ground: put the machine in action, a charge takes place, and the two plates are held together by a great force. Indeed, it is difficult to separate them without fear of breaking one or both. Having accomplished their separation, and applied their surfaces to the electroscope, I find both sides of one of the glass plates *positive,* and both sides of the other *negative*. The two plates when together had obviously operated as one plate only. I now again put them together as before, and by applying the discharging rod to the two coatings a discharge takes place, and the plates easily separate.

I charge the two plates when again in close contact, and on trial find they are held fast together. I now turn the negative side upwards, and connect it with the prime conductor, and a moment's action of the machine neutralizes the electrization of the plates, and they are easily separated; but if the action of the machine be continued too long, the plate becomes electrized the reverse way, and are held together as before.

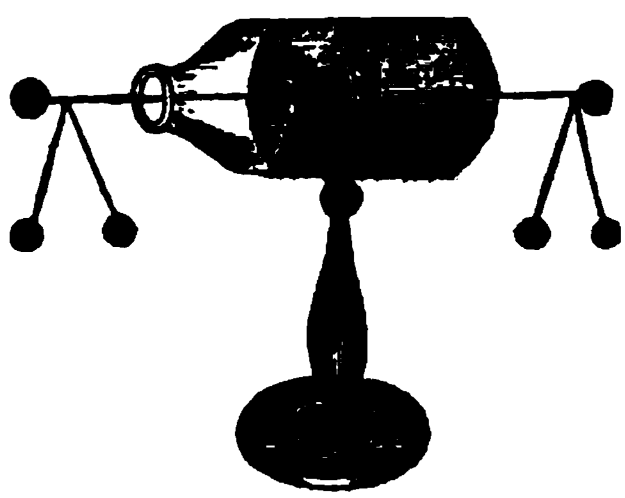
This is an old experiment, first made known by the father Jesuits at Pekin to the academy at St. Petersburg, in the year 1755. It was extensively investigated by Mr. Symmer in this country, and by M. Cigna and Father Beccaria in Italy, who were led to several other interesting experiments in consequence.

To insure success it is necessary that the glass plates have perfectly flat surfaces, and by being square the corners of the one can be placed across the sides of the other, thus giving a better opportunity of separating them when charged.

Mr. Symmer made a great number of experiments with black and white silk stockings, which, when one of each colour was worn on the same leg for half an hour, and both taken off together without separating them, showed but feeble signs of electric action; but on separating them afterwards they were found to adhere together with great force, and a crackling noise was heard and sparks seen all the time. When quite separated, and one held up in each hand, the repulsion in each stocking was so powerful that it stood out in full shape as if the leg were in it. The attraction between the two stockings was powerful, and a spark was seen as they rushed together. When one stocking was in the other, a force of fifteen pounds was required to separate them. The white stocking was always electro-positive.

The experiments with the two glass plates lead to the explanation of another curious fact attending charged glass generally. When a

plate of glass, coated on both sides, is charged to a high intensity, and then brought over the cap of an electroscope, the gold leaves are but little affected whichever surface be turned towards the instrument, although several sparks have been thrown on the positive surface, each of which if thrown on a metal surface of the same extent would have affected the electroscope to a greater degree. The glass is intensely charged, but its accumulated fluid on the positive side presses *inwards* to a much greater extent than outwards, because of the negative surface offering a less resistance than the pressure on the other side; and the forces are principally *engaged* in the substance of the glass, there being but a small portion *disengaged* to operate on external bodies. If, however, either of the coated surfaces be uninsulated for a moment, a portion of the previously engaged force is relieved, and the opposite side of the glass now affects the electroscope much more than before. This doctrine is beautifully illustrated by operating on an insulated Leyden jar, furnished with a pith-ball electroscope on each surface, as represented in the figure. When the jar is charged, and removed from the prime conductor for a few moments, both pairs of balls diverge a little, but not much, for reasons already stated. I now touch the outer coating, and its balls immediately collapse, being uninsulated; but the other pair of balls diverge to a great extent, because my finger has supplied a small portion of electric fluid to the outer surface, and *relieved* a corresponding quantity on the inner surface, which now being *disengaged* in the glass, springs to the balls which it repels.



I now take away my finger from the outer surface, and place it on the ball belonging to the inner one. By this means I take away the previously released fluid, and also a little more; and the balance of forces through the glass is again broken, and the negative balls diverge in quest of that portion last taken away from the inside. I again change the position of my finger, and the outside balls collapse, and the inside pair diverge as at first; then by a series of contacts with my finger, first with one coating and then with the other, I gradually discharge the jar.

There are several other methods of discharging a jar gradually and silently, but none more effectually than by a pointed wire. If, for instance, the ball were removed from the stem of a jar it exposes a sharp point, and on this point being applied to the prime conductor the charge is accomplished in the usual way: now remove the jar and place it on the table, and its charge will issue from the point into the air; and in a short time the jar will become perfectly neutral. When this experiment is made in a darkened room, a beautiful purple brush of light is seen issuing from the point, and by close attention the brush appears largest and brightest at first, and gradually

grows smaller and feebler till the discharge is complete. When both sides of the charged jar are insulated, and each furnished with a pointed wire, the star will be seen on the negative point during the whole time that the brush appears on the positive point. These are beautiful theoretical facts, though by no means adapted for the lecture room.

When the discharge of a jar is made in the usual way through a metallic circuit, the velocity of the fluid is too great to allow of measurement: or rather, perhaps, there are circumstances in the way which frustrate all attempts to ascertain its velocity. When, for instance, a discharge is made on one end of a long metallic circuit, the entering fluid disturbs all that belonging to the conducting wire, a portion of which leaves the wire at one end of the circuit at the time the new fluid from the jar enters the other end; and this fact is, perhaps, the most formidable barrier against obtaining satisfactory experimental results. During lightning, however, the electric fluid may be traced by the eye as it traverses the air through long striking distances, and obviously occupies an appreciable period of time.

I have a beautiful experiment to bring forward, which has been often used as an illustration of the great velocity of the electric fluid whilst traversing metallic conductors, but for reasons already explained it is of no value in that capacity.

I have about fifty yards of iron wire chain suspended round the room by silken cords, and through this chain I discharge the battery of jars from a high intensity. The chain, you will have observed, was splendidly illuminated throughout by brushes of scintillating fire, which sprang simultaneously from every link in the circuit. The results of experiments of this kind are interesting, if it were on no other account than by their showing that a quantity of the electric fluid is thrown into the air from every sharp point in the circuit. The scintillating of the iron, however, adds much to the brilliancy of the display.

MEETING OF THE BRITISH ASSOCIATION.

(Selections continued from page 223).

Mr. Nasmyth brought forward several specimens to illustrate the remarks which he intended to make in further illustration of his remarks on Friday. From late accidents, arrising from breaking axles, the public were alive to the subject, and it was desirable that the question should be examined. In locomotive engines the axle was the chief point of danger; and it was therefore important, both as a scientific and practical question, to determine the nature and habitude of iron when placed under the circumstances of a locomotive axle. Experiment was the only way to discover this, and he would have wished to place iron under exactly similar cir-

cumstances; but the short time intervening since Friday had rendered it impossible to do so. One opinion on Friday was that the alternate strains in opposite directions which the axles were exposed to, rendered the iron brittle, from the sliding of the particles over each other. To illustrate this, Mr. Nasmyth took a piece of iron and bent it forward, it broke in six bends. He had suggested annealing as a remedy for this defect: in proof whereof, he took a piece of annealed wire, which bore eighteen bends, showing an improvement of three to one in favour of annealing. He should therefore advise railway companies to include in their specification that axles should be annealed; he did not like oppressing engineers with useless minutiæ in specifications, but this was so useful and so cheap, that he considered it ought to be insisted on. To exhibit on a larger scale the effect produced on iron in our workshops, he showed a specimen of iron as it came from the merchant: being nicked with a chisel, it broke in four blows with a sledge, at the temperature of sixty degrees, with a crystalline fracture; by raising the temperature forty degrees higher, it bore twenty blows, and broke with the fibrous or ligneous fracture; so that the quality of iron was not the only circumstance to be considered as influencing the fracture. I noticed also, said Mr. Nasmyth, on Friday, the injurious effect of cold swaging, as causing a change in the nature and fracture of the iron; and here let us take the practical workshop view of the case, and not run after the *ignis fatuus* of electricity or galvanism, but consider the practical effects. Swaging was necessary in many cases, for instance, when an axle had collars welded on, these could not be finished with the hammer, and certain tools called swages were used, from the action of which great condensation of the iron took place, and a beautiful polish was given to the surface, with what injurious effect he would show by the next specimen, which had been heated red hot, and then swaged till cold; it broke at one blow without nicking, and the fracture was very close and beautiful, like steel. This showed the fallacy of considering close fine grain a good test of excellence in wrought iron; but moderate swaging was often necessary, and not injurious, unless where an over regard to finish carried it to excess. To prove that annealing restored the toughness and fibrous texture, a portion of the last bar was heated, and cold-swaged till cold as before, then heated dull red, and left to cool gradually; it bore 105 blows without breaking, and at last was rather torn asunder than broken, as was shown by the specimen; this proved that the fibrous structure was restored by annealing, and he therefore thought it should be insisted on in specifications. The effect of heating to welding-heat was very injurious, unless the iron was subsequently hammered to close the texture; a piece of the same iron heated to welding, and left to cool, broke without nicking, in one blow, showing very large crystals, especially in the centre. The effect of nicking was also very singular. The strength of iron was generally stated to be equal to its sectional area; but a nick not removing $\frac{1}{100}$ of the area took away $\frac{1}{10}$ of the strength.

Mr. Naamyth broke a piece of nicked, or rather scratched wire, to illustrate this point. These, and similar things, did not prove that science and practice were at issue; but, as Halley reached the great accuracy of his prediction of the return of his comet by taking into account the disturbing forces of Jupiter and Saturn, and the other planets amongst which the body had to pass, so scientific men should seek in the workshops correctional formulæ, by learning there the practical occurrences which would elucidate their theories, and he hoped that these specimens might be of some use.

Prof. Willis was aware that many subjects of a purely physical nature could only be explained by practical research; and one great advantage of the British Association was, that it brought scientific and practical men together for this purpose.—Mr. Fairbairn was of opinion, that the two chief causes of breaking axles seemed to be bending and percussion, changing the fibrous into the crystalline structure; this last was the effect of cold swaging, and he hoped that his friend Mr. Hodgkinson would undertake a series of experiments on this very interesting subject. By nicking a bar the *extended* fibres were cut, which supported more of the weight than the *compressed.*—Mr. Worthington thought the additional friction in steps, given by annealing, would counterbalance the advantage gained in strength, as case hardening (the very opposite operation) was used to diminish friction, by giving a glassy hardness to the surface, the annealed axles would be laid aside after a few trips, from the friction: he would wish, as a security for life, that the springs should be made as long as possible, to diminish the effect of concussion.—A member shewed specimens of pins which had been broken in machinery. They appeared very crystalline in fracture; the bar from which they had been made was fibrous and tough: he showed also specimens of tender axles broken on the Sheffield and Rotherham railway. Tender axles are most frequently broke from the action of the brakes on the wheels: crystals larger in the centre of the axles than at the ends.—Mr. Mallet was quite at issue with the French committee on the very uncomfortable Report which they had made so authoritatively; he believed that the alternate strains, as long as they were within the limit of elasticity, did not injure the texture of the iron. Wire might be bent backward and forward to infinity, if we kept within this limit. The effect of nicking depended on a change of crystalline structure; that the effect of the nick in determining fracture, was according to the sharpness of the chisel, and the direction; a nick sloping, according to the natural direction of the fibre, was not so efficacious; a molecular change was effected by this cutting across the fibres: we, in fact established a plane cleavage in the iron; this took place in glass when scratched with a diamond, although glass, from passing through the intermediate viscous state, did not crystallize so definitely as iron, which crystallizes *per saltum.* Iron, polished and placed in such a situation as just not to corrode, if scratched, immediately began to corrode; and iodide of mercury presented a curious example of entire disintegration

from a slight scratch. Crystallization takes place in the direction of motion; in rolled iron the motion was in the direction of the length of the bar or plate, and percussion, in a direction perpendicular to that had the effect of breaking up these laminæ or fibres of crystals, into their original molecular arrangement; and this effect was proportionate to the temperature caused, and extent of motion imparted. But he believed, to effect this molecular alteration required more violence than was to be expected in any ordinary railway travelling, or, indeed, any circumstance of machinery in perpetual work. The chief danger was to be feared where any cutting perpendicular to the direction of the fibre took place, as, for instance, shafts, with square collars, while a little rounding out preserved them. That rotation of iron induced magnetism, he was aware, but he did not believe that either rotation or vibration would affect iron which was sound when first applied. If this theory were correct, the engineer should discard wrought iron entirely; no engineer was safe, no suspension bridge should be trusted.—Sir J. Robison considered that injuries did arise from vibration and alternate bending; he instanced tongues of musical instruments, and the effects of bending pure tin, which crackled and broke when very slightly bent in opposite directions.—Mr. Mallet believed those tongues to be alloyed, and he found that alloys altered their crystalline nature from mere lying by, as tough brass became brittle, &c., which did not happen in simple metals.—Mr. Nasmyth showed that the effect of hammering bars was actually to make them hollow; every stroke had a tendency to make the bar an ellipse, and the intersection of all their axes was apt to be a hole, from the sliding of the laminæ over each other.

Mr. Fairbairn read his Report "On Experiments on the Transverse Strength of hot and cold blast Iron."—The bars, as described in the former Reports, were supported by standards, 4 feet 6 inches apart, and were loaded with different weights; they were occasionally carefully examined, and showed a very slight progressive deflection. He had no doubt that they would ultimately break, but the progress was very slow. He read a table showing the weights laid on, and the deflections of each bar.

Mr. Hartopp said, that Mr. Fairbairn's former experiments on hot and cold blast iron, had created a false impression with regard to the strength of hot blast iron. Mr. Fairbairn had found very little difference between the hot and cold blast; but his experiments, made with great accuracy, and in which the weights were laid on with great care, were of little practical advantage, as these were not the circumstances under which iron was tested in practice: there percussion, violent and sudden impact, should be expected, and here lay the great deficiency of hot blast iron. Even in Mr. Fairbairn's experiments, Oldberry, No. 2, cold blast, bore twice the percussion of Oldberry hot blast; and Milton hot blast was only half the strength of Elsicar cold blast, made of the same ore and smelted with the same coal. Experiments had been made in Yorkshire with great

care; the results being Low Moor cold blast bar iron, three inches diameter, broke with 6 blows, ditto Scrap, 3 blows, ditto hot blast, 1 blow; again, Low Moor cold blast 18 blows, Bierly ditto, 18, hot blast of as good materials, 3 blows; again, Elsicar cold blast 21 blows, Milton hot blast $1\frac{1}{2}$ blow; therefore, in iron for axles this difference of at least $\frac{1}{6}$ of the strength was very important. As to scrap iron it bore too high a character. Scrap, made on the old plan, was all charcoal iron, but the modern scrap iron was very inferior, being 32s. 6d. per ton cheaper, so that iron-masters put off as much of this cheap material as possible. Hot blast iron was rejected now for water pipes, &c., and even for cannon balls; and, in fine, he had been told by very eminent marine engine makers, that where any percussion took place, hot blast cast iron was only half the strength, and wrought iron only one-sixth the strength of cold blast.—Mr. Fairbairn explained, that he had found great difficulty in obtaining specimens from the different iron-masters, who would of course send, when possible, the best specimens, but every care had been taken to insure accuracy in the experiments.—Mr. Hodgkinson said, that the average strength of hot blast had been $\frac{2}{100}$ weaker than the cold, but the inferiority was chiefly in the softer irons; as the hardness increased the two kinds approached to equality, and in the hardest irons the hot blast was the best. He thought his experiments, made without any interest on either side, and with the greatest care, were more to be depended on than experiments made by those who had an interest in the result.—Prof. Vignoles explained, that the question of hot and cold blast had nothing to say to the late contract for cannon balls.

Mr. Hodgkinson then explained his apparatus for trying the strength of materials. He brought his apparatus forward as he had made many experiments; and he was desirous to render them as trustworthy as possible, by convincing the members that every care had been taken to insure accuracy. Other experiments had been rendered unworthy of reliance from injudicious methods of affixing the testing apparatus—as those of Rennie and Capt. Brown on iron; Girard's experiments, &c. In crushing specimens, it was necessary that both ends should be well bedded and the pressure transmitted through the axis. To this, other experimenters had not always attended, and by using the pressure of bores directly on the substance to be crushed, they introduced the different errors arising from the pressure being oblique, transmitted through the side, or being exerted on mere points, instead of equably exerting its force over the entire top surface: to obviate these objections, he had devised apparatus by which all these errors were avoided. Mr. Hodgkinson explained the crushing apparatus by drawings, &c. In experiments on tearing asunder, he had also taken great care, by means of apparatus which he exhibited and explained, that the strain should be through the axis, and otherwise free from causes of error. Mr. Hodgkinson explained his experiments on torsion, and

illustrated his observations throughout by many models and specimens of the substances on which the experiments had been made.

Prof. Mosley asked whether, in the experiments on beams, care had been taken to obviate the effects of the friction of the beams on the supports, as this would affect the direction of pressure, altering it from vertical to inclined, and the neutral line only passed through the centre of gravity of the beam when the pressure was vertical; also if care had been taken in laying on the weights, as a weight suddenly laid on produced mathematically twice the effect in deflection. Theoretically, the weight should be increased by small additions, even as the grains of sand.—Mr. Hodgkinson said he had taken all precautions with regard to the weights; they were added by small portions, and with great care; the beams rested on tolerably smooth cast iron, on which he believed the friction would be of little importance.

Mr. Brockedon exhibited specimens of his patent India rubber stoppers for bottles, explaining the late improvements in the construction of the cores on which the India rubber is spread. The present cores, he said, were made of cotton twisted into strands, &c., by means of a machine which he explained by a diagram, the cylindrical rope now consisted of several strands of tightly twisted cotton, lapped with flax thread, and laid together longitudinally, loose fine cotton rovings being placed between them; the entire was then lapped in a cylindrical form with a flax thread, attaining by this method the advantages of perfect roundness and firmness; they also gave sufficient hold to the corkscrew, and bore the heating process well. These stoppers would slide on glass when wet, but not when dry (although there was no cohesion in this latter state), so that the bottler, by slightly wetting these stoppers with the liquor which he was bottling, could easily insert them; and when this slight film of moisture was dried up, the stopper required considerable force to withdraw it.

The President presented to the section a pamphlet, transmitted for the acceptance of the British Association, from M. Lenz, containing two essays, one "On the Resistance of the Human Body to Galvanic Currents," the other "On the Theory of Magneto-Electric Machines."

Sir D. Brewster made a communication "On Crystalline Reflection," which he said, was a mere notice connected with the undulatory theory of light. Having (said Sir David), in a conversation with Professor Kelland, had my attention directed to Professor M'Cullagh's interesting memoir on the laws of crystalline reflection and refraction, I have felt it necessary to make a communication on the subject to the British Association. In consequence of the results which I laid before the Bristol Meeting, Professor M'Cullagh was led to revise the views to which he had been led by my earlier experiments in 1819. I had at that time the advantage of communicating with him personally and by letter; and, having preserved

copious abstracts of his paper on the subject, I did not look into the memoir itself till yesterday, when my attention was drawn to the following note:—"I was at this time in doubt whether that phenomena observed with oil of cassia could be reconciled to the theory; and when the note in page 36 was written, I was almost certain that they could not. But I have since, I think, found out the cause of this perplexity: some of Sir David Brewster's experiments were made with natural surfaces of Iceland spar; others with surfaces artificially polished. I believe (though I have made very few calculations relative to the point), that the former class of experiments will be perfectly explained by the theory; *the latter I am certain cannot, nor ought we to expect that they should;* for the process of artificially polishing must necessarily occasion small irregularities by exposing little elementary rhombs with their faces inclined to the general surface, and the action of these faces may produce the unsymmetrical effects which Sir David Brewster notices as so extraordinary. If this does not account for such effects, I do not know what will." Had Prof. M'Cullagh communicated to me this explanation of the incapacity of the undulatory theory to account for the extraordinary unsymmetrical phenomena which I described to the British Association, and which exist to a much greater extent than I described; or had it been contained in the two abstracts of his memoir, with which I was familiar, I could at once have removed the difficulty referred to in the preceding note. The view he has taken of the action of an artificially polished surface of Iceland spar, is a mistaken one. The exposure of elementary rhombs with faces oblique to the general surface, would show themselves in separate rays inclined to the principal pencil, especially in solar light. It could not for an instant be overlooked by an experienced observer. Such faces I can produce at pleasure, by a slight chemical action upon the surface, whether polished by crystallization or by art; and it is impossible to confound the pencil which they reflect, with that which is given by the general surface. It is useless, however, to pursue this argument any farther, because I have obtained exactly the same results in using natural faces, and in using artificial ones, and especially on planes perpendicular to the axis of the crystal, where I have found the same results with the natural faces of the *Chaux carbonatée basée* of Hauy, and with those produced by artificial grinding. In this case, the coincidence is still more remarkable, as the very friction of the finger is capable of developing on this surface the faces of elementary rhombs; but the reflections from these never disturb in the slightest degree the physical action of the general surface. I have no doubt, that Prof. M'Cullagh will concur in the accuracy of these views, and, with that candour which distinguishes him, will acknowledge, as he has almost done already in the preceding note, that the undulatory theory is, generally speaking, incapable of explaining the phenomena of crystalline reflection. A late writer, who seems to believe in the omnipotence of the undulatory theory, has ventured to aver,

"that the theory of Fresnel has actually remanded back experiment to read her lesson anew, and convicted her of blindness and error." Although we are not sensible of having enjoyed such benefits, or suffered such reproach, yet we are convinced that even false theories and imperfect generalizations have often sent back to their studies the most sagacious observers. But such benefits have, doubtless, been often mutual; and if the interchanges of intellectual aid had not always been equal, the more liberal contributor might have acknowledged it in a more courteous manner.

Sir W. Hamilton: I ventured to express a wish, some years ago, that Sir D. Brewster would publish those important experiments which he has made on this subject.—Sir D. Brewster: My reason for not publishing anything on the subject is, that I have not arrived at anything like an approximation to a law. I have only made a mere collection of disintegrated facts.—The President hoped Sir D. Brewster would put the public in possession of his experiments; for, whether the undulatory theory was capable of explaining them or not, they might enable mathematicians to grapple with the phenomena.—Prof. M'Cullagh said, that he had himself presented to the Royal Society, some time since, a paper on this subject, in which he had followed out pretty similar views to those entertained by Sir D. Brewster.

"On a very curious fact connected with Photography, discovered by M. Möser, of Königsberg," communicated by Prof. Bessel to Sir D. Brewster.

Sir D. Brewster said, he was requested to communicate an account of some remarkable facts connected with the theory of photography. A new process of producing photographic impressions had been discovered by Dr. Möser, of Königsberg: and an account of the discovery had been brought to this country by Prof. Bessel, who received it from the discoverer himself. The subject was most important, and it would have been a great misfortune if the Physical Section had separated without being made acquainted with it. The following were the general facts connected with it:—A black plate of horn, or agate, is placed below a polished surface of silver, at the distance of one-twentieth of an inch, and remains there for ten minutes. The surface of the silver receives an impression of the figure, writing, or crest, which may be cut upon the agate, or horn. The figures, &c., do not appear on the silver at the expiration of the ten minutes, but are rendered visible by exposing the silver plate to vapour, either of amber, water, mercury, or any other fluid. He (Sir D. Brewster) had heard Prof. Bessel say, that the vapours of different fluids were analogous to the different coloured rays of the spectrum; that the different fluids had different effects, corresponding to those of the spectrum; and that they could, in consequence of such correspondence, produce a red, blue, or violet colour. The image of the *camera obscura* might be projected on any surface,—glass, silver, or the smooth leather cover of a book,—without any

previous preparation; and the effects would be the same as those produced on a silver plate covered with iodine.

This paper gave rise to an animated conversation, in the course of which M. Bessel said that he had seen some of the pictures taken by this process, which were nearly, but not quite, as good as those obtained by Mr. Talbot's process.—Sir D. Brewster said, this was the germ of one of the most extraordinary discoveries of modern days; by it there seemed to be some thermal effect which became fixed in the black substance; and not only so, but M. Bessel informed him, that different lights seemed to affect different vapours variously, so that there seemed to be something like a power of rendering light latent; a circumstance which, if it turned out so, would open very new and curious conceptions of the physical nature of light; on the emission theory, it would be easy to account for this; on the undulatory theory, he could not conceive how it could be possible.—Prof. M'Cullagh said, he believed Newton had somewhere thrown out a suggestion, that luminous particles, as they entered into bodies, might be caught and retained, within certain bounds, by continual attractions.—Sir D. Brewster said, that the experiments which he had performed with nitrous gas, seemed to strengthen some such view as this, for at certain temperatures, we had here an instance of a gaseous body as impervious to light as a piece of iron.—Sir J. Herschel thought it a pity to encumber this new and extensive field of discovery now laid open to them, by any speculations connected with the theory either of undulations or emissions. He had found that paper could be so prepared, as that the impressions of some colours might become permanent upon it, while others were not; and thus it became possible to impress on it coloured figures by the action of light. He exhibited to the section a piece of paper so prepared, which, at present, had no form of picture impressed on it, but which was so prepared, that, by holding it in a strong light, a red picture would become developed upon it. He wished much he could prevail on Sir W. Hamilton to explain to the section a metaphysical conception which he had disclosed to him, and which seemed to him, though darkly he owned, to shadow forth a possible explanation of many difficulties.—Sir W. Hamilton said, that, appealed to by Sir J. Herschel in this manner, he could not avoid placing before the section the theory alluded to, however imperfect and obscure. He then explained it; but we regret our inability to express it adequately. It appeared to depend on the conception of points, absolutely fixed in space, and endowed with certain properties and powers of transmission, according to determined laws.—Prof. M'Cullagh had indulged in speculations allied to, and, as he conceived, involving this very conception of Sir W. Hamilton, and had even followed out some of its consequences, by reducing it to a mathematical form: the conception was of double points, or poles, transmitting powers; but he had abandoned it as mere speculation.—Sir D. Brewster thought

these speculations tended to repress experimental research, and to turn men's minds from what was solid to what was fanciful. He conceived also that indulgence in them, and mere abstract mathematical research, by rendering men averse from the more humble and laborious pursuits of experiment, absolutely produced a distaste for these subjects; and to this he attributed the fact that, while learned societies frequently overlooked, and even refused to publish in their transactions experimental papers, the transcendental flights were always sure to find a welcome place.—Sir J. Herschel considered that there could be no true philosophy without a certain degree of boldness in guessing; and such guessing, or hypothesis, was always necessary in the early stages of philosophy, before a theory has became an established certainty; and these bold guesses, in their proper places, he conceived, should be encouraged, and not repressed. Sir W. Hamilton's conception, he thought, perfectly clear in its metaphysics, and should not be thrown overboard merely because it was metaphysical.—The President hoped that Sir W. Hamilton would develope and publish this speculation, in order that it may be sifted, scrutinized, and rejected if merely ideal, or established and adopted, if solidly founded in nature and fact.

Sir D. Brewster next made a communication "On the Geometric Forms, and laws of Illumination of the Spaces which receive the Solar Rays, transmitted through Quadrangular Apertures." He said his attention was called to this subject by an accidental discussion on the point, whether or not Aristotle, in explaining the circularity of images formed by quadrilateral apertures, employed the appropriate idea when he said, that those images were, to a certain extent, quadrilateral, but appeared circular, from the eye being unable to recognize faint impressions of light. Prof. Whewell, in his "History of the Philosophy of the Inductive Sciences," had distinctly stated, that Aristotle had not used the appropriate idea, and that the question was entirely a geometrical one, the appropriate idea being the rectilinear nature of light. Having been led accidentally to consider the subject, he (Sir D. Brewster) had determined in a simple manner the form of the aperture at all distances, and had been led to take the same view of the subject with Aristotle, who seemed to have employed the appropriate idea.

Mr. Goodman "On the Causes of Dissimilarity in the Frictional and Voltaic Electricities, with Remarks on the Decomposition of Water by the former, and on Magnetism."—The author argued in this paper, that the electricity of the common electric machine differs from that of the voltaic battery, inasmuch as the fluid is in the former case in a state of tension; in the latter in a state of intensity; or, as it may be otherwise expressed, the two active forces (*i.e.* the antagonist electricities) are in one case separated from each other, while, in the other, they are continually subject to each other's contiguous influence, attraction, or polarization.

Mr. Mallet read a paper, "On the Action of Air and Water on Iron." This the third Report for which the Association is in-

debted to Mr. Mallett. The object of former tabulated results was to determine the actual loss by corrosion in a given time, and the comparative durabilites of rust of the principal "makes" of cast iron of Great Britain, and to discover on what durability depended. The tables of experiments now presented show, that the rate of corrosion is a decreasing one in most cases; and that the rapidity of the corrosion in cast iron is not so much dependent upon the chemical constitution of the metal as upon its state of crystalline arrangement, and the condition of its constituent carbon. The present report, too, extends the inquiry to wrought iron and steel, of which between thirty and forty varieties have been submitted to experiment. The results show that, the rate of corrosion of wrought iron is in general much more rapid than that of cast iron or of steel. The finer the wrought iron is, and the more perfectly uniform in texture, the slower and the more uniform is its corrosion. Steel corrodes in general more slowly, and much more uniformly, than wrought or cast iron. The results of the action of air and water in the several classes of iron have been examined and chemically determined. The substance spoken of as plumbago was next described. It is produced by the action of air and water on cast steel, especially that in the raw ingot, in the same way as it is in the case of cast iron. A quantity of plumbago, found in the wreck of the "Royal George," absorbed oxygen on exposure to the air with such rapidity that it became nearly red hot. Mr. Mallet next described a method of protecting iron by a modification of the zinc process. It was found impossible to cover the surface of iron with zinc, to which it had no affinity. The first process was to clean the surface of the iron, taking off the coat of oxide, and then immersing it in double chloride of zinc and ammonium, which covered it with a thin film of hydrogen, by which its affinity for the zinc is much increased. The iron was then covered with a triple alloy of zinc, sodium, and mercury. Mr. Mallet produced several specimens of his alloy, one of a bolt to be driven into a ship's side, and another a cannon shot covered with his preparation, and exposed to the weather on the roof of a building, and which was perfectly preserved. Cannon balls were so much oxidized by exposure to atmospheric influences that in five or six years they become useless. The French Institute had been engaged in experiments to protect these, and had tried zinc, but had been compelled to abandon it. Mr. Mallet also brought under the notice of the Section a method of preventing the fouling which takes on the bottoms of iron ships, especially in tropical climates, by means of which invention he had ascertained that plants and animals were prevented from adhering to the ship's bottom. According to Mr. Nasmyth's theory, corrosion on railways is checked by the trains passing in one direction, and takes place when they pass in both directions. Mr. Mallet had made some experiments in order to determine this point, which were not yet complete; but he was inclined to think that the difference between the two cases were apparent and not real. He

was continuing his investigations, and hoped to report further on a future occasion.

Dr. Schunk read a paper "On Hæmatoxylin, the Colouring Principle of Logwood," by Prof. O. L. Erdman, of Leipsic.—The Hæmatoxylin used by the author in his experiments was prepared by the process of charcoal. In a state of purity, hæmatoxylin is not red; it is in itself no colouring matter, being merely a substance capable of producing colouring matters in a manner similar to lecanorin, orcein, or phloridzin. The colours which it produces are formed by the simultaneous action of bases (particularly strong alkalies), and of the oxygen of the atmosphere. By the action of these it undergoes a process of eremacausis, which, after forming colouring matters, ends in the production of a brown substance resembling mould. The colour of hæmatoxylin varies from a pale reddish-yellow to a pale honey colour. The crystals are transparent, possess a strong lustre, and may be obtained a few lines in length. Their form is a rectangular four-sided prism, sometimes with a pyramidal summit. The taste of hæmatoxylin is similar to that of liquorice. With excess of ammonia, it forms what the author calls hæmatein, analogous to orcein, &c.

"On an Economical Voltaic Combination of extraordinary power," by F. W. de Moleyns, Esq. The author stated that, while the discoveries in electro-magnetism gave promise of its ultimate application as a motive power far surpassing steam, it was matter of much importance to discover a mode of charging or giving attractive power to *soft iron*, at a cost which should render it as a mechanical agent generally available. The voltaic arrangement now produced to the Section, the author believed would be found to possess in a very great degree those advantages so much desired for the proper developement of electro-magnetic energy. The combination consisted of an acidulated solution of nitrate of ammonia, in contact with platina, solution of muriate of ammonia, and zinc; the nitrate solution being separated from the muriate by a diaphragm of wood, biscuit-ware, or other porous substance not acted upon by the liquids. The acidulated solution was thus prepared: six ounces of nitrate of ammonia are dissolved in two fluid ounces of soft water, and this solution is then combined with an equal quantity, by measure, of the pure sulphuric acid of commerce, adding the acid gradually, the vessel containing the mixture being kept in a frigorific preparation, so as to prevent the heat evolved exceeding 100 degrees. The muriate of ammonia is dissolved in soft water to saturation. The zinc is *not* amalgamated, and the use of *cast* zinc is to be avoided. The platina is the thinnest foil that can be procured, but the author found that box-wood cut to the thickness of veneer, and charred on each side *superficially*, might be substituted, and used with equal advantage. The author stated that, with a voltaic combination consisting of half a fluid ounce of the acidulated nitrate solution, one ounce of the

saturated solution of the muriate of ammonia, a strip of platina foil three inches by two, surrounded by a piece of sheet zinc of equal surface, he had succeeded in supporting a weight of 2,000 lb. with an electro-magnet of the horse-shoe form, measuring sixteen inches from pole to pole, and three-fourths of an inch in diameter, and that the attractive force, before contact, was in proportion.

"On a Peculiar Condition of Iron," by Prof. Schöenbein."—In this paper the learned author continued his researches upon the different effects produced by iron in its active and passive states. Without a detailed description of the apparatus employed in the production of the singular phenomena observed by the Professor, and which our space will not permit us to give, it is impossible to make the phenomena themselves intelligible. They are intimately connected with the curious fact, discovered by the author, of the property which iron possesses with respect to oxygen, *i.e.* in certain conditions to be an oxidable, in others a non-oxidable metal.

"On a new method of Analyzing Cast Iron and other Metallic Carburets," by Dr. Ure.—The method proposed by Dr. Ure is similar to that of Regnault and Dr. Bromeis, with the exception that he employs pure chlorate of potash in the combustion, instead of a mixture of that salt with chromate of lead, and collects the resulting carbonic acid in a peculiar pneumatic apparatus, filled with diacetate of lead, instead of the potash apparatus of lead.

"On the Composition and Characters of Caryophylin," by Dr. Lyon Playfair.—The author pointed out the most advantageous method for obtaining caryophyllin. He mentioned that only a small quantity could be derived from cloves by a direct process; but that, by a protracted digestion with alcohol and the exposure to the air, a considerable quantity might be procured from *Caryophyllus aromaticus*. Dumas and Ethling had assigned the formula $C_{20}\ H_{1}\ O_2$ to this substance. Dr. Playfair stated, that although this is the correct expression of the composition of *melted* caryophyllin, it is not so of the substance in its natural state. He found the empirical formula of caryophyllin dried for several days at 212°, to be $C_{40}\ H_{33}\ O_5$, or the rational formula $C_{40}\ H_{32}\ O_4 \times HO$. A considerable heat is required to expel this water in the open air, but it escapes at a moderate heat in vacuo.

Sir J. Robinson explained Mr. Prosser's method of making earthenware or porcelain from dry powder or clay compressed. The advantage was that no twisting or alteration of shape (excepting a little shrinkage) took place in the burning. From the accuracy with which articles formed by compression retained the shape of the mould, they could be fitted together very easily and smoothly. Sir J. Robinson showed a piece of tesselated pavement made of these tiles, which, although just laid together without cement, was perfectly smooth on the surface. He wished particularly to introduce to their notice a roofing tile of construction novel in this country: from the peculiar manner in which these tiles overstepped

each other, a little Roman cement rendered them perfectly watertight; and from their not being absorbent, they were not liable to exfoliate, and would, therefore, be almost imperishable. The old form of tile weighed about 105lb. per square yard, while this only weighed 58lb. They were manufactured at Stoke-upon-Trent.

Mr. J. S. Russell explained his "Indicator of Speed of Steam Vessels." This was a simple application of a well known principle; it was not novel, but he had applied it successfully, although others had failed. It depended on the hydrodynamical fact, that if a reservoir be filled with water to a certain height, the water will flow from an orifice at the bottom with a velocity proportionate to the height; and conversely, if the reservoir be empty and this orifice turned towards stream, the water will rise in the reservoir proportionate to the velocity. His plan was to pass a tube through the bow of the vessel, and carry it along the flooring to the centre of gravity of the vessel, where it terminated in a vertical glass tube, exhibiting the weight of water within. To this tube there was attached a moveable scale, the zero of which being placed on a level with the point at which the water stood when the vessel was at rest, the rise of the water in the tube when the vessel was set in motion exhibited the velocity at which the vessel was passing through the water. He had tested the accuracy of this indicator by sailing vessels at least twenty times over a measured distance of 15⅔ miles, and comparing his tube with Massey's log, the common log, calculations from the number of strokes, &c., he found it more accurate than any. By putting a stopcock in the pipe just under the glass tube, he was enabled to regulate the orifice until the greatest regularity was obtained, and he could now depend on the indications within the twentieth of a mile. From these experiments he had constructed a scale, which he exhibited, and of which the following is an extract; the first column exhibiting the speed in miles per hour, and the second the height of the water in the tube above the zero line, expressed in feet:—

Miles per hour.	Feet on the scale.
15	7.5625
13	5.6800
11	4.067
9	2.722
7	1.647
5	0.84
3	0.3025
1	0.0336

Prof. Vignoles read a communication "On the use of Béton and Concrete in the construction of Breakwaters." The use of béton had greatly increased in France of late, especially in marine works; it was similar to concrete, but not exactly identical with it. Béton, like concrete, was composed of lime mixed with broken stones, gravel and sand; but it was supposed to require hydraulic lime,

while concrete in this country was frequently made of common lime when not to be exposed to the action of the sea. Béton was first introduced in France by Belidor, and lately much advocated by Vicat; since then it had been much used, and he considered that attention was due to the use that had lately been made of it in the Port of Algiers by M. Poiteul, the engineer of that harbour.

Mr. P. Taylor had witnessed the complete success of this system at Marseilles, where a very difficult structure had been accomplished in this way The original béton was a cement made of lime and ground brick; this made a very good cement; cubic masses of this concrete, 10 feet on the face, were used. These cubic masses were formed at Marseilles on the very brink of the precipice over which they were to be rolled into the sea. He was now constructing four bridges, and he was as confident in placing his foundation on béton as on stone, he had so often witnessed the efficacy of the plan—one point was that in France they had very good hydraulic lime.—Mr. Thompson said, that when good hydraulic lime was not to be procured, he had used the chalk lime of the north of Ireland, which, though useless by itself, afforded a good cement when mixed with clay. The lime was burned and ground, the clay was ground and well mixed with the lime; this was then burned in a kiln, and the mortar was quite satisfactory, but too expensive for common use.—Mr. Smith (of Deanston) had lately directed his attention to the same subject. Concrete might also be used to make tiles for drying land when clay was dear and fuel was expensive; hollow tubes of concrete might be made in the drains by using proper cores, and constructing them in lengths of 3 or 4 feet, pouring the concrete round the core, leaving small apertures for the admission of water. He considered that they might be made on a larger scale, so as to be very serviceable for sewers.—Mr. Bateman had been compelled to resort to artificial lime for a large water work in Ireland; he tried M. Vicat's experiments over again, and found that they succeeded perfectly in experiment, and even on the large scale while the pressure of water was moderate, but when the depth reached twelve feet, he found the lime was completely washed away where unsupported, and even the floor of the culvert was at length disintegrated by the action and pressure of the water: from this he concluded that Vicat's experiments should not be implicitly trusted in great pressures of water.—Sir M. I. Brunel explained the great cohesion of bricks, when interlaid with hoop-iron or laths beaten into a fibrous state.

Mr. L. Schwabe explained his method of spinning glass, and brought forward specimens of the glass thread, and also of the cloth woven; he showed the spinning machine with which this was effected, and also displayed many other filamentous substances from which he had succeeded in fabricating cloths, Assam silk, fibres of the pinna, &c.—Sir J. Robison mentioned that the strength of this silk in proportion to its lightness, was such that Mr. J. S. Russel

had used it for the towing line of his small models, as it could be strained without breaking until the deflection was only half an inch, while brass wire of the same length, &c., broke when the deflection was three inches.

Mr. West then read Mr. Shaw's paper "On a New Steam Engine worked with three kinds of pressure, viz. action of high pressure steam, the expansion of steam, and the atmospheric pressure caused by its condensation." The description was illustrated by models and drawings. The lower part of the piston fits the cylinder and is steam tight; the upper part or plunger, in the form of a hollow cylinder, is longer than the cylinder and passes through a stuffing box in its cover. The piston rod rises through the middle of the plunger, and is connected with the parallel motion: the valve is a modification of the single slide, the upper passage leading to the upper part of the cylinder, the middle to the lower part of the cylinder, and the lowest to the condenser. When the piston is descending the valve admits steam to the annular space between the plunger and cylinder, when, the middle and lower passages being open, the direct pressure of the atmosphere upon the plunger, the high pressure steam upon the annulus, and an additional pressure of one atmosphere upon the annulus from the vacuum beneath, concur to produce the down stroke; during this the steam is cut off from the cylinder, and the communication between the cylinder and condenser is shut; the steam, which before occupied the annulus, now acts against the plunger for the ascending stroke, while the whole force of expansion, the annular surface of the piston being then passive.—Mr. West then described Mr. Shaw's hydraulic engine, of which a model and drawings were also exhibited. In this engine the want of elasticity in water, which formed a great objection to the application of a fluid to a piston is supplied by the elasticity of air in a chamber communicating with the cylinder; it is well fitted for situations where the fall is great, but the supply too small or the space too limited for the use of a water-wheel, as in mines.

Mr. Clegg then explained his dry Gas-meter. It acted on the principle of the differential thermometer, in which a difference of temperature between two bulbs partly filled with alcohol, was shown by the rising of the spirit in one and depression in the other. He had taken advantage of this principle by suspending two little glass vessels, partly filled with alcohol and connected by a tube, and by passing the gas over heaters, it warmed one of these bulbs, and the spirit was driven into the other, which, becoming the heaviest, swung to the bottom of the arc in which the vessels vibrate; here it, in its turn becoming warm, was emptied of its spirit, and thus becoming lightest was in its turn displaced; the continuance of these oscillations marked the flow of gas, and being registered by the usual train of wheel-work, the number of vibrations had been proved, by many careful experiments at all seasons of the year, to afford an accurate measure of the quantity of gas.

THE ANNALS

OF

ELECTRICITY, MAGNETISM,

AND

CHEMISTRY;

AND

GUARDIAN OF EXPERIMENTAL SCIENCE.

DECEMBER, 1842.

Experimental Researches on a peculiar Action of Iron upon Solutions of some Metallic Salts. *By* Dr. C. F. Schöenbein.

(*Continued from page* 413.)

"Some time ago I published several papers, in which I made known some very remarkable facts regarding the action of iron upon oxygen. According to the notions generally adopted by philosophers respecting the action of metals performing the function of the positive electrode upon oxygen set free by voltaic action, iron, as one of the more readily oxidable metals, chemically combines with that element. In one of the papers alluded to, I have shown that these notions with regard to iron, do not hold good in all cases, and that this metal acquires under certain circumstances, the property of platina or gold, that is to say, that whilst constituting the positive electrode, it is neither oxidized nor otherwise chemically affected by oxyacid solutions, which usually act upon iron with more or less violence. I have further observed, that this inactivity of iron depends upon the manner of closing the circuit, as well as upon the chemical nature of the electrolytes contained in the solutions in which the polar wires of the pile are immersed. Solutions containing oxyelectrolytes which act chemically upon iron, as, for instance, sulphuric or nitric acid, require the circuit to be closed in a certain manner, in order to evolve oxygen at the positive iron. Solutions containing oxyelectrolytes which do not sensibly act upon iron, as, for instance, those of potash, soda, and a great many oxysalts, allow the evolution of oxygen at the positive iron quite independently of the manner of closing the circuit. In solutions containing, besides oxyelectrolytes, others of a different nature, for instance, hydracids, haloid salts, &c., no evolution of oxygen takes place in whatever manner the circuit may be closed. From these facts, and others stated elsewhere, I am inclined to infer that the affinity of iron for

oxygen is destroyed by a current moving through the metal in a certain direction, and that the affinity lost in this way by the iron is revived by an opposite current. To ascertain whether this view holds good generally with regard to iron, I have made a series of experiments, the results of which are as follows. I introduce an iron wire which had previously been connected with the positive pole of a small battery, into an aqueous solution of the common sulphate of copper, by means of which the circuit was closed. According to my hypothesis, no action whatever is to take place on the part of the iron upon the solution, under the circumstances here stated. My expectations were fully realized, for after many hours' action of the pile, not the smallest particle of copper was deposited on the iron wire, its surface had not undergone the least change, and during the whole time of action oxygen was evolved at the iron. But as soon as the passage of the current through this metal was opposed only for a moment, for instance, by taking out of the copper solution either the negative polar wire or the positive one, there appeared on the surface of the latter a film of copper. The same result was obtained by joining momentarily the two polar wires within the solution, or by touching the iron wire with another metal capable of precipitating copper.

"Now, by these facts I think two things are clearly shown: first, that iron ceases to have any affinity for the oxygen both of the oxide of copper, and of the water decomposed by voltaic action: and secondly, that this state of chemical indifference lasts only so long as there is a current passing from the iron into the copper solution. This influence of current electricity upon the chemical bearings of iron is highly interesting, not only on account of its being contrary to the electro-chemical notions hitherto entertained on the subject, but also on account of the circumstances under which the oxygen resulting from the decomposition of water is presented to iron. These circumstances are, indeed, of such a kind as highly to favour the oxidation of the metal, for oxygen in a nascent state is brought into contact with iron: and there is at the same time a portion of the acid of the salt set free by voltaic action at the iron wire, which also tends to occasion the oxidation of the latter. The same remarks apply to the fact already stated, that iron is not acted upon by nitric or any other oxyacid, provided this metal is placed under the influence of the pile in the manner above mentioned. Iron, however, may acquire the property of being not acted upon by nitric acid or solutions of certain metallic salts without being subjected to the influence of a current. This remarkable fact has been observed by Sir John Herschel, and more recently by Mr. Faraday and myself. If, for instance, a common iron wire, having been made inactive by repeated immersions in common nitric acid, is put into a solution of blue copper vitriol, not the least chemical action takes place. It is true that it happens sometimes that such a wire precipitates copper at the moment of its being plunged into the

solution, but in such a case the inactive state of the metal had ceased previously to immersion. Now whether iron is or is not in its peculiar condition may easily be ascertained by the appearance of the surface of that part of the wire which had been immersed in nitric acid. If the surface is bright, the wire is inactive; if yellowish brown, the metal has assumed its common state, and will consequently act upon the copper solution in the usual manner. The peculiar condition of iron with regard to the copper solution can be destroyed in different ways. In the first place it may be destroyed by making an inactive iron vibrate. If such a wire is wetted by the said solution, and afterwards rather violently struck against any solid body, for instance against a table, immediately after the shock a film of copper will make its appearance along the whole wetted surface of the wire. According to the results of my experiments, published in several periodical works, inactive iron is rendered active with regard to nitric acid by the same means. In the second place the active state of iron may be reproduced by touching the inactive metal with a metal which acts chemically upon the solution of the copper salt. If an inactive wire is wetted by this solution, and then touched on any point of the part wetted, with a piece of common iron, zinc, cadmium, tin, lead, arsenic, or even copper, the precipitation of copper instantaneously takes place at the point of the iron wire where contact had been effected, and this action rapidly extends itself over the whole part of the wire which is covered with the solution. It is a matter of course that the same effect can be obtained by touching the inactive iron wire within the solution of the copper salt with the same metals. But the peculiar condition of iron may be changed into the common state without immediately touching those parts of the metal which are surrounded with the copper solution. If, for instance, an inactive wire is put into the solution, so as to allow part of it to rise above the level of the fluid, and if a wire of any of the metals above mentioned is placed in the same solution, having likewise one of its ends raised above the surface of the liquid, copper will be precipitated as soon as the free ends of both wires are made to touch one another. This mode of changing the state of iron is exactly the same as that by which a similar change of condition of this metal may be effected with regard to nitric acid. Now, all these facts evidently prove that the peculiar condition of iron, whatever the cause of it may be, is always destroyed by the chemical action of metals brought into contact with iron when in the inactive state. There is certainly one singular fact, which seems to indicate as if contact independently of and unconnected with chemical action could of itself occasion a change of state in iron. It has been already stated, that copper brought in some of the ways mentioned into contact with an inactive iron wire, which is immersed in the copper solution, renders the latter metal active. Now copper of course cannot be precipitated from the solution of blue vitriol by copper: the chemical action of this metal upon the copper salt must,

therefore, be essentially different from that which is exercised by the more readily oxidable metallic bodies in question. First, I thought there might, perhaps, be some free acid contained in the solution, and by this means chemical action occasioned. To ascertain the correctness of this view, I added ammonia to the solution until flakes of oxide of copper were beginning to make their appearance; but the copper wire acted in such a neutral solution in the same manner as it did in the more acid one; chemical action consequently does not result from the cause supposed. I think there is only one way left to account for the fact in question. It is well known that copper put into a solution of a salt containing the deutoxide of this metal, unites by degrees with this base, to form protoxide of copper. Although this chemical action is extremely slow and weak, still it is of sufficient power to revive in the inactive iron its dormant affinity for oxygen.

"There is no doubt that, one case excepted, in all others hitherto mentioned, in which passive iron is rendered active, an electric current is produced, passing from the metal in which chemical action originates, through the solution, into the inactive iron, and from this back again to the first metal. It is further obvious, that the direction of the current passing through the inactive iron is opposite to that in which the current moves through an iron wire which performs the function of the positive electrode of a pile. The chemical effects produced upon iron by these different currents being also the reverse of one another, it seems to me that these facts speak in favour of the idea already suggested, that the chemical affinity of iron for oxygen is destroyed by one kind of current, and called forth again by the other. It is true, one of the most sagacious philosophers of the age, Mr. Faraday, has started an idea which seems to account very satisfactorily for the phenomena in question. According to this view the peculiar condition of iron depends either upon a film of oxide covering the metal, or upon a relation of oxygen to iron equivalent to oxidation, so that the particles forming the surface of the inactive iron have satisfied in one way or other their affinity for oxygen. Applying the same hypothesis to account for the bearing of iron in the solution of blue vitriol, which Mr. Faraday has made use of for explaining the singular action of iron upon nitric acid, we must say, an inactive iron wire does not act upon the solution of the copper salt, because there is no immediate contact between the truly metallic particles of the wire and the said solution, on account of the interposing film of oxide, or something similar to it. But if now another metal be put into the solution, which is chemically acted upon by the latter, there is a current produced, proceeding from the active metal, and passing through the solution into the inactive iron. By this current water is decomposed, hydrogen evolved at the iron; the film, or what is equivalent to it, deprived of its oxygen; and by this means a truly metallic surface of the iron wire produced. Though this way of accounting for the facts in

question recommends itself by its beautiful simplicity, and what is still more valuable, by the great advantage of bringing back an apparently anomalous case to a general law, still there are weighty reasons stated by me elsewhere, which will hardly allow the adoption of Mr. Faraday's sagacious hypothesis.

" After having examined the action of iron upon the common sulphate of copper, I was curious to see how the same metal acts under similar circumstances upon the solutions of the nitrates of mercury. Before entering into details upon the subject, I must not omit to state, that I did not observe any essential difference of action between the protonitrate and pernitrate of mercury. A common iron wire, cleaned or not, when put into a solution of either of the neutral nitrates of mercury, does not act in the least upon the salt, that is to say, no mercury is precipitated on the iron; but what is still more surprising, the iron wire, after having been immersed only for a few seconds in such a solution, shows all the properties of inactive iron; it will, for instance, not be acted upon by common nitric acid, nor by a solution of blue vitriol. Even when a strong solution of the mercurial salt is diluted with 1,000 times its volume of water, it will still render an iron wire inactive, though in this case, as might be expected, some time is required for obtaining the effect. But if a common iron wire is first immersed in water containing so little of nitric acid as scarcely to change the colour of blue litmus paper, and afterwards plunged into the solution of mercury, it will precipitate the latter metal. It is, indeed, quite extraordinary how far this influence of the acids, favouring metallic precipitation, extends. I mixed a strong solution of neutral protonitrate of mercury with 1,000 times its volume of water, and in the same proportion I diluted common nitric acid. By putting the wire first into the acidulated water, it always acquired the property of decomposing the diluted solution of mercury on being plunged into it. Common muriatic acid, even 4,000 times diluted with water, produced the same effect. Though it is a well known fact that some free acid contained in metallic solutions favours the precipitation of one metal by another, still I am not aware that any chemist has as yet stated any particulars regarding the extent and cause of this influence. The peculiar action of acids mentioned seems to be intimately connected with the subject of my researches on the action of iron upon nitric acid, and to afford a case similar to that presented by inactive iron, in its bearing to strongly diluted nitric acid. In one of my published papers on the subject, I have stated that inactive iron loses its peculiar condition by being put into diluted nitric acid; the same thing takes place in the case before mentioned. Common iron wire is of itself inactive in a solution of a neutral salt of mercury, but is rendered active by being subjected to the action of acidulated water previously to its immersion in the solution. According to Mr. Faraday's views the acid must produce the effect spoken of, by cleaning the surface of the wire; that is to say, by dissolving some

film, with which even a common wire must be supposed to be covered; but for reasons before alluded to, I cannot entertain the opinion of this distinguished philosopher, even in this case.

"The view I have taken of the subject leads me to ascribe the effect in question to chemical excitement in the metal occasioned by the acidulated water. As iron having only for a few moments been immersed in diluted acid decomposes the neutral solution of mercury, it might be supposed that this metal should act in the same manner in a solution made somewhat acid. But I found this not to be the case. A solution of pernitrate of mercury obtained by saturating nitric acid, sp. gr. 1·35, with peroxide of mercury, was mixed with its own volume of the same acid. A common iron wire put into this acid solution had no action upon it, and assumed its peculiar condition. I could put even twenty volumes of nitric acid to it, without producing any action. But I must not omit to state the singular fact, that there is in this respect a great difference between a wire which is cleaned and one which is not. If, for instance, a common iron wire has only once passed through a piece of linen or cloth, it will be acted upon by the acid solution containing only one volume of nitric acid, whilst an uncleaned one is not affected at all. This difference is the more remarkable. as an uncleaned wire is much more violently attacked by mere nitric acid than a clean one. Another fact, still more singular, is, that different parts of the same piece of wire are sometimes differently acted upon by the same acid solution of mercury, one part being, for instance, entirely inactive whilst another contiguous to it proves to be highly active. I call this fact a very singular one, because every bit of a whole roll of iron is acted upon in common nitric acid. When an iron wire, cleaned or not, is plunged into the solution of mercury containing from thirty to fifty times its volume of nitric acid, it will be affected, and continue to be acted upon if left in the solution; but when it is again taken out of the fluid and held for hardly a second in the air, after its re-immersion it will prove entirely inactive. It is surprising, that almost the same results are obtained at very different degrees of temperature. I heated a mixture containing twenty volumes of nitric acid, and one volume of the solution of pernitrate of mercury to its boiling point. The end of an iron wire put into it was certainly acted upon, but by withdrawing it only for a few moments from the solution it was rendered inactive, so that it could afterwards be re-immersed in the nearly boiling acid fluid without being attacked by it. A certain proof that the metal acquires, even at this high degree of temperature, its peculiar inactive state is, that when put into a solution of blue vitriol or into mere common nitric acid, it does not in the least act upon these substances. In making these experiments I frequently observed the curious fact, that the iron wire immersed in the nearly boiling acid solution loses its inactive condition as soon as it is a little raised so as to expose to the air a very small part of that portion of

wire which has been immersed in the fluid; but though this is often the case, it is not invariably so.

"The results which I have obtained from experiments made with iron wire and an acid solution of mercury much diluted by water, are likewise worthy of being stated. One volume of a very strong solution of neutral protonitrate of mercury, five volumes of nitric acid, sp. gr. 1·35, and 200 volumes of water were mixed together. A piece of cleaned iron wire put into this solution did not precipitate mercury. By plunging such a wire into water slightly acidulated, its power of acting upon the salt of mercury, as above mentioned, is instantaneously called forth. The wire having once acquired this power retains it; provided, however, it be called into play at intervals of time not much exceeding a second or so. But if the wire after having been active in the solution is taken out of it, cleaned from the adhering mercury, and left exposed to the air only for a few seconds, it will have lost its property of precipitating the last-named metal, and rest entirely inactive in the solution, whatever length of time it may remain immersed in it. This remarkable and sudden change of the condition of iron is most likely due to some action of the air; for if the wire, being still in its active state with regard to the solution of mercury, is put into water or hydrogen gas, it preserves its precipitating power. I have not yet put iron into other mediums than those mentioned, nor have I examined whether moisture has anything to do with the phenomenon. At any rate this subject seems to me in many respects sufficiently interesting to deserve further investigation. Before passing to another subject, I have still to mention some facts connected with those just spoken of. An iron wire which proves to be entirely inactive in the last mentioned solution of mercury, is not so with regard to a solution of blue vitriol or to common nitric acid; for a wire which does not throw down mercury, precipitates copper, or is violently acted upon by the said acid. From chemical reasons we are led to expect that the very contrary should take place: the affinity of copper for oxygen being much greater than that of mercury; that is to say, we should think the mercury salt easier to be decomposed by iron than the copper salt. It seems, therefore, as if the anomalous fact does not result from the action of common affinity. Another fact worthy of remark is, that iron acts quite differently upon the neutral nitrates of mercury dissolved in alcohol or ether, from what it does upon the aqueous solutions of the same salts. In the former case iron always precipitates mercury and never turns inactive, whilst, as above stated, in the latter case the contrary takes place. If an iron wire, having been rendered inactive by immersion in an aqueous solution of the mercury salt, is put into alcohol or ether containing the same salt, it loses its peculiar condition and returns into its active state.

"I think it not quite irrelevant to the subject treated of in this paper, if I produce a new case bearing evidence in favour of the

theory, according to which voltaic electricity is due to chemical action. It is true that the beautiful researches of Mr. Faraday, as well as those of Mr. de la Rive, have led to results which remove from an unbiassed mind even a shadow of a doubt on the subject, and which prove in the most satisfactory manner, that mere contact of heterogeneous metals is not capable of disturbing their electrical equilibrium. Still, as the number of philosophers who maintain the hypothesis of Volta is as yet rather considerable, I think it not quite useless to increase the body of evidence against it.

"If an iron wire rendered inactive by immersion in nitric acid is associated with a platina wire, and two of their ends put into a solution of blue vitriol, not the smallest quantity of copper will be precipitated on the platina; but if the inactive iron is thrown into chemical action, by being touched within the solution, either with a common iron wire, or by any other metal which chemically acts upon the copper salt, at the very moment of contact a film of copper makes its appearance on the platina. Now, if according to the views of Volta, electricity be excited by the mere contact of different metals, in the case in question a current should be produced, and in consequence of such a current chemical decomposition should take place, that is to say, copper should be eliminated at the platina. But from such not being the case, it follows that there is no current, consequently no electricity, produced by the contact of iron and platina. By having recourse to the galvanometer, the absence of a current under the circumstances mentioned, is placed beyond doubt. If the inactive iron wire is connected with one end of the wire of the galvanometer, the platina wire with the other one, and if the two free ends of the iron and platina wires are plunged into a solution of blue vitriol, not the least deflection of the magnetic needle takes place; but as soon as the part of the inactive iron wire immersed in the solution is touched with a metal capable of causing chemical action, the needle becomes agitated, and at the same time a deposition of copper takes place on both wires. From this fact it appears that the oxidation of iron has no sooner been occasioned than two effects of a current are produced: chemical decomposition of an electrolyte and affection of the needle. Now, as previously to oxidation no such effects are obtained, we are fully entitled to draw the inference that the phenomenon of oxidation bears to that of a current the relation of cause to effect, or generally speaking, that voltaic electricity is due to chemical action, and by no means to contact.

"I am quite confident that inactive iron can be used in a great number of cases for obtaining results similar to that just spoken of, and that the peculiar state of this metal offers to philosophers in many other respects a most valuable means for making electro-chemical researches."

Professor Schöenbein's next paper is entitled "On the peculiar Voltaic Condition of Iron as excited by Peroxide of Lead."

The author commences by stating "that the most powerful vol-

taic association into which iron can be brought in order to excite its peculiar condition, is that with peroxide of lead. A common iron wire, one of the ends of which is covered with this substance, proves to be inactive, not only towards nitric acid of a given strength, but towards nitric acid containing *any quantity* of water; whilst my oxidized iron wire, or one associated with platinum, &c., is acted upon by that acid, if much diluted, in precisely the same manner as unprotected iron. But the superiority of the association mentioned to any other at present known, is exhibited in a still more striking manner by putting the two ends of an iron wire (one of which is covered with peroxide of lead) into an aqueous solution of common sulphate of copper. Under these circumstances not the smallest particle of copper will be precipitated on any part of the wire immersed in the solution. This peculiar state of the wire, however, lasts only as long as both ends of it are in the solution; for no sooner is the protected one removed from the liquid, than the other left immersed turns active, and throws down copper. In this respect, therefore, there is a great difference between the action of the wire in question upon the solution of blue vitriol, and that of the oxidized one upon common nitric acid. This difference of action implies another, which is the impossibility of transferring, within the copper solution, the peculiar state from wire to wire, which is so easily accomplished in nitric acid. I must not omit here to state the remarkable fact, that by mixing the solution of the sulphate with a comparative small quantity of common salt, the calling forth of the peculiar state is prevented, not only in the foregoing case, but in all that will be mentioned hereafter. This fact is by no means an insulated one, and depends upon the same cause as that which proves to the disengagement of oxygen at the iron whilst in the capacity of the positive terminal of the pile, from a solution of haloid salts, &c. Presuming that by rendering iron inactive towards sulphate of copper in the way described, a current would be excited in the same direction as that produced by calling forth the peculiar state of this metal in nitric acid, and having had recourse to the galvanometer, I was very much struck on finding that the needle was not in the least affected, the instrument employed in my experiments, though indicating rather feeble currents, certainly does not possess the greatest possible sensibility. It consists of 100 convolutions of wire. Should it be discovered, by the employment of more delicate galvanometers, that no current is called into existence during the process of iron being rendered inactive, it would prove that the inactivity of iron has, as to its origin, nothing to do with what we call a current."

"The best way," says Professor Schöenbein, "of associating iron with peroxide of lead, is to make it the positive *electrode* of a *couronne des tasses*, of about a dozen pairs of copper and zinc, and to put the free end of this wire into a solution of the common acetate of lead for about eight or ten minutes. By the action of the pile

the oxide is deposited on the positive iron wire. Many reasons lead me to believe, that iron associated with this substance, will form the most powerful voltaic element known, and I am just about to construct a pile of such couples."

[*Remarks.*—We think that nothing more is wanting than the above statements to show, that the author was perfectly unable to determine the cause of the phenomena he had observed : and that he did not know whether electric currents were produced or not: and we must add, that nothing in science is more certain than that the whole were of electric origin, nor nothing more easily discovered in researches of this kind, than the existence of electric currents in the professor's experiments.—Edit.]

In a letter addressed to Dr. Faraday, Professor Schöenbein states :—

"From a series of experiments lately made by me with the view of ascertaining the voltaic relations of some peroxides, platina, and inactive iron to one another, I have obtained results, which, in my opinion, are such as to throw some additional light upon the cause of voltaic electricity, and to modify, to a certain degree at least, the notions we have hitherto entertained about that interesting subject. The voltaic relation of peroxide of lead to iron engaged my attention some time ago, and in Poggendorff's 'Annalen,' I have stated that the peroxide in question, if voltaically associated with iron, disappears by degrees when plunged into nitric acid of any strength. Now as we know that no chemical action whatever takes place under the circumstances mentioned, iron being in its peculiar condition, and having, in a voltaic point of view, all the properties of platina, I could not but be very much surprised at the disappearance of the peroxide of lead. Although I was not able to trace at the time any voltaic current, or to account for any disturbance of the electric equilibrium of the arrangement alluded to, I nevertheless suspected that the solution of the substance mentioned was effected under the influence of current electricity. Having now at my disposal a galvanometer which is provided with 2,000 coils, and made in other respects very delicate, I have taken up that subject again, and attempted first to ascertain whether there is any voltaic relation of platina to inactive iron. In contradiction to the results obtained some time ago, I have found by means of my galvanometer, that iron, being in its peculiar condition and associated with platina, gives rise to a sensible current if put into nitric acid, be the latter ever so strong or somewhat diluted with water. Making use of an acid of sp. gr. 1·4, the deviation of the needle (on putting the iron and platina wires in connection with the galvanometer) amounted to about 90°. I must not omit to state that the current excited under the circumstances mentioned is not a momentary but a continuous one, and at the same time quite independent of any oxidation of the iron. The direction of the current in question is such as it would be if the latter metal was attacked by the acid, that is to say, inactive

iron is positive to platina. Another fact, as curious and interesting as that just stated is the following one. Two platina wires being connected by one set of their ends with the galvanometer, and by the other set with nitric acid or an aqueous solution of sulphate of copper, excite a current, provided one of the ends (immersed in the fluid) of one of the platina wires be covered with a film of peroxide of lead. The current passes from the platina through the fluid to the peroxide: when the said film is so thin as to produce what are called 'Nobili's colours,' it disappears within a very few seconds after having been immersed in nitric acid, and the whole arrangement connected with the wire of the galvanometer. From the facts stated it appears, that platina is positive with regard to peroxide of lead, and that the disappearance of that compound is caused by a current which eliminates hydrogen at the negative peroxide, by which means the latter is reduced to protoxide of lead, and rendered soluble in nitric acid. In a similar manner I have ascertained that the voltaic relation of inactive iron to peroxide of lead is exactly the same as that of platina to the said peroxide. In using peroxide of silver instead of that of lead, voltaic effects are produced quite the same as those which were just spoken of, that is to say, a continuous current is excited, to which the peroxide acts the part of the cathode, and either of the metals in question that of the anode. As to the voltaic relation which one of the peroxides mentioned bears to the other, my experiments have shown that peroxide of silver is always negative with regard to the peroxide of lead, be the fluid made use of nitric acid or a solution of blue vitriol. Now, from all the facts above stated, I think we may be allowed to draw two important inferences: 1. That peroxide of silver, peroxide of lead, platina, and inactive iron constitute a series of substances, in which the preceding one is always negative with regard to that which follows in the list. 2. That any two of the four substances mentioned being voltaically associated with one another, and put either into nitric acid or a solution of sulphate of copper, excite a continuous current, which is not due to oxidation or any chemical change. It is hardly necessary to add, that the currents produced under the said circumstances are extremely feeble, being only indicated by most delicate galvanometers.

"The facts spoken of are highly important in a scientific point of view, as they seem to produce evidence in favour of that theory which asserts, that by the mere contact of heterogenous substances their electrical equilibrium can be disturbed quite independently of any chemical action taking place between them. All chemists certainly maintain, that pure nitric acid, for instance, does not chemically affect at all either platina or peroxide of lead; and inactive iron too, as we now well know, is not in the least attacked by the said acid. Now, I ask, whence does the current originate which is produced when we combine the substances in question in such a manner as to form with them a voltaic arrangement?

"I have attempted to answer this puzzling question in a paper which before long will be published in Poggendorf's 'Annalen,' as well as in the 'Biblioth. Univ.,' and in which you will find besides a detailed account of all the experiments made by me upon the subject spoken of. If my time was not so much taken up with a variety of business, I would have drawn up a memoir in English, and sent it to the editors of the 'Philosophical Magazine,' for insertion; but those gentlemen will, perhaps, give a translation of the paper.

"Allow me to communicate in a general manner the view which I have taken of the subject in question. In the first place I am by no means inclined to consider mere contact in any case as the cause of the excitement of even the most feeble current. I maintain, on the contrary, in accordance with the principles of the chemical theory, that any current produced in a hydro-electric voltaic circle is always due to some chemical action. But as to the idea which I attach to the term 'chemical action,' I go further than M. de la Rive seems to go; for I maintain, that any tendency of two different substances to unite chemically with one another, must be considered as a chemical action, be that tendency followed up by the actual combination of those substances, or be it not; and that such a tendency is capable of putting electricity into circulation. I will try to render this idea of mine somewhat clearer by applying it to some particular cases. Supposing a voltaic circle to be composed of platina, peroxide of lead, and nitric acid; I say that the current excited in such an arrangement is due, first, to the tendency of the acid to unite with the protoxide of lead, or, what is the same, to the tendency of one proportion of the oxygen to separate from the peroxide; secondly, to the tendency of water to combine with the same protoxide to form a hydrate; and thirdly, to the tendency of water to withdraw a proportion of oxygen from the peroxide to produce peroxide of hydrogen, which tendency, from very well known chemical reasons, is yet increased by the presence of the acid. It is true that none of the said tendencies do lead to any chemical result, for no nitrate of lead, no hydrate, no peroxide of hydrogen is actually produced; but are we allowed to infer, from the want of a practical result, that no chemical action whatever takes place when nitric acid and peroxide of lead are put in contact with one another? I ask, are we to suppose that the chemical affinities alluded to are entirely dormant, and incapable of any exertion? The results from my late experiments induce me to answer the question in the negative. Being quite of opinion, that chemical affinity and current electricity are but different forms of the same thing, I cannot help thinking that any sort of chemical action or tendency must be capable of being transformed into the shape of a current. For that current which is produced by inactive iron, being voltaically associated with platina, I likewise account by a chemical tendency on the part of the former metal. Though inactive iron be not in the least

attacked by nitric acid, its affinity for the oxygen of the latter is, on that account, by no means entirely destroyed; the metal whilst surrounded by the acid is continually tending to oxidize itself, and the current excited in such a case is nothing else but as it were the electrical translation of a chemical exertion.

"All the cases above stated, where currents are observed independently of any chemical change, can be explained by applying to them the same principles as that by means of which I have accounted for the current produced by nitric acid and peroxide of lead, &c.

"According to my experiments, peroxide of silver proves to be the most powerful means for exciting in iron its peculiar voltaic condition. It surpasses in this respect even the peroxide of lead. An iron wire, for instance, one end of which is covered with only a small particle of the first-mentioned substance, will not be attacked either by nitric acid of any degree of dilution, nor by a solution of blue vitriol. The voltaic association of one substance with the other is easily effected by connecting one end of an iron wire with the positive electrode of a pile, and by plunging for a few minutes the other end of the wire into a solution of nitrate of silver."

The next paper of Professor Schöenbein that has appeared in this country is headed "On the Polarization of certain Solid and Fluid Substances."

"I read a paper containing an account of the results of my researches on the voltaic polarization of solid and fluid bodies, before the scientific meetings which took place at Bâle and Fribourg, some few months ago.

"This memoir, I think, will soon be published in the 'Biblioth. Univ.' and in Poggendorff's 'Annalen,' and the scientific public thereby enabled to appreciate the facts related in it. The sort of investigations alluded to could not but lead me to make numerous experiments similar to those which were mentioned the other day in the French Academy, as performed by MM. Matteucci and Peltier, of which I have, however, up to this present moment, but a very imperfect knowledge. Having from want of time not yet been able to draw up a regular paper on the results of my most recent voltaic researches, and thinking them not quite void of scientific interest, I beg the favour of your giving in the forthcoming number of your widely circulated journal a place to the general statements, the tenor of which is as follows:—

"1. A platina wire polarized either in the positive or negative way loses its peculiar condition by being heated red hot. I call positively polarized a wire which has acted for some time the part of the negative electrode in water slightly acidulated by sulphuric acid; and I term negatively polarized a wire which has in the same liquid performed the function of the positive electrode.

"2. A platina wire positively polarized loses its peculiar condition by being plunged only for a single moment into an atmosphere of chlorine.

"3. A platina wire positively polarized loses likewise its electromotive power by being placed in an atmosphere of oxygen; but in order to destroy entirely the polarity of the wire, it is necessary that it should remain for some seconds in the gas mentioned.

"4. A platina wire negatively polarized loses its peculiar condition by being put into an atmosphere of hydrogen, but in order to obtain this effect, it is required that the wire in question should remain for some seconds in the gas.

"5. A platina wire polarized either negatively or positively is not sensibly affected by being placed in an atmosphere of carbonic acid, or in one of any other gas which does not chemically act either upon hydrogen or oxygen.

"6. A platina wire (in its natural state) assumes in every respect the condition and voltaic bearings of a positively polarized wire by being plunged only for a few seconds into an atmosphere of hydrogen.

"7. Gold and silver are not sensibly affected under the same circumstances.

"8. A platina wire does not acquire any degree of electromotive power by being put into oxygen gas: the metal remains in its natural state. Neither is any degree of such power acquired by gold or silver under the same circumstances.

"9. Platina, gold, and silver, by being placed only for a few seconds in an atmosphere of chlorine, assume the voltaic state of a negatively polarized wire.

"10. Water slightly acidulated with sulphuric acid and holding some hydrogen dissolved, bears to acidulated water containing no hydrogen the same voltaic relation that zinc does to copper; provided, however, both fluids be separated from each other by a membrane, and connected with the galvanometer by means of platina wires. If for the latter purpose (that is to say, for connecting the fluids with the galvanometer) gold or silver wires are made use of, the said fluids do not excite the least current.

"11. Two fluids, one being acidulated water containing some oxygen dissolved, the other being likewise acidulated water containing no oxygen, appear to be in a voltaic point of view perfectly indifferent to each other, whether they are connected with the galvanometer by platina, silver, or gold wires.

"12. Water slightly acidulated with sulphuric acid and holding some chlorine dissolved, bears to acidulated water not containing any chlorine, the same voltaic relation that copper bears to zinc. In other terms, the former fluid acts under certain circumstances the electromotive part of the peroxides of silver, lead, &c.

"13. The aqueous solution of hydrogen mentioned in § 10, loses its property to excite a current by being mixed with a certain quantity of an aqueous solution of chlorine; and *vice versa*, the latter fluid loses its electromotive power mentioned in § 12, by being mixed with a sufficient quantity of hydrogen dissolved in water.

"14. Muriatic acid positively polarized loses its peculiar voltaic condition by being mixed with some chlorine, and the same acid being negatively polarized loses its polarity by being treated with some hydrogen. From the facts stated, and others which are mentioned in the memoir above alluded to, a great number of rather important inferences might be drawn; but having for the present no leisure time to do so, I am obliged to confine myself to stating those which follow:—

"*a.* The secondary currents produced both by polar wires, electrolytic fluids, and secondary piles, are due to chemical action, *i.e.* (in the cases mentioned), to the union of oxygen with hydrogen, or to that of chlorine with hydrogen; and not as M. Peltier seems to think, to the mere act of the solution in water of the gasses mentioned.

"*b.* The chemical combination of oxygen and hydrogen in acidulated (or common) water is brought about by the presence of platina in the same manner as that metal determines the chemical union of gaseous oxygen and hydrogen.

"*c.* The current produced by a platina wire being surrounded by a film of chlorine, or by water holding chlorine in solution, is not dependent on the action of the latter body upon platina, but on the action of chlorine upon the hydrogen of water.

"*d.* Electrotytic bodies do not suffer even the weakest current to pass through them without undergoing decomposition. This inference is drawn from the fact ascertained by me some time ago, that platina wires, acting as electrodes in muriatic acid, are polarized by a current so weak as not to be able to electrolyze even iodide of potassium.

"*e.* The most delicate test to ascertain that electrolyzation has taken place, is the polarized state of the electrodes.

"I cannot close this communication without pointing out the beautiful, and, as it seems to me, most conclusive evidence in favour of the correctness of the chemical theory of galvanism, now so much contested, which is afforded by the fact stated in § 10. If the mere contact of the two different fluids mentioned there, were the real cause of the current obtained, it is obvious that the same current ought to be produced whether the fluid be connected with the galvanometer by means of gold, or if they be connected with the instrument by that of platina wires; but the result being determined by the nature of the connecting wires, and platina being known to favour the union of hydrogen and oxygen, whilst gold and silver do not possess in any sensible degree that property, we are entitled to assert that the current in question is caused by the combination of hydrogen with [the] oxygen [contained dissolved in water], and not by contact."

The last paper of Professor Schöenbein's on these subjects, that we shall have to notice, will be found at page 285, vol. vii, of these "Annals."

Bale, December, 1838.

Observations and Suppositions towards forming a new Hypothesis for explaining the several Phenomena of Thunder Gusts. By Dr. FRANKLIN.

1. NON-ELECTRIC bodies that have electric fire thrown into them, will retain it till other non-electrics that have less approach; and then it is communicated with a snap, and becomes equally divided.

2. Electrical fire loves water, is strongly attracted by it, and they can subsist together.

3. Air is an electric *per se*, and when dry will not conduct the electrical fire; it will neither receive it, nor give it to other bodies; otherwise no body surrounded by air, could be electrified positively and negatively: for should it be attempted positively, the air would immediately take away the overplus; or negatively, the air would supply what was wanting.

4. Water being electrified, the vapours arising from it will be equally electrified; and floating in the air, in the form of clouds, or otherwise, will retain that quantity of electrical fire, till they meet with other clouds or bodies not so much electrified, and then will communicate as before-mentioned.

5. Every particle of matter electrified is repelled by every other particle equally electrified. Thus the stream of a fountain, naturally dense and continual, when electrified, will separate and spread in the form of a brush, every drop endeavouring to recede from every other drop. But on taking out the electrical fire they close again.

6. Water being strongly electrified (as well as when heated by common fire), rises in vapours more copiously, the attraction of cohesion among its particles being greatly weakened, by the opposite power of repulsion introduced with the electrical fire; and when any particle is by any means disengaged, it is immediately repelled, and so flies into the air.

7. Particles happening to be situated as A and B, Fig. 5, Pl. II, representing the profile of a vessel of water, are more easily disengaged than C and D, as each is held by contact with three only, whereas C and D are each in contact with nine. When the surface of the water has the least motion, particles are continually pushed into the situation represented by A and B.

8. Friction between a non-electric and an electric *per se*, will produce electrical fire; not by creating, but collecting it: for it is equally diffused in our walls, floors, earth, and the whole mass of common matter. Thus the whirling glass globe, during its friction against the cushion, draws fire from the cushion, the cushion is supplied from the frame of the machine, that from the floor on which it stands. Cut off the communication by thick glass or wax,

* Thunder gusts are sudden storms of thunder and lightning, which are frequently of short duration, but sometimes produce mischievous effects.

placed under the cushion, and no fire can be produced, because it cannot be collected.

9. The ocean is a compound of water, a non-electric, and salt, an electric *per se*.

10. When there is a friction among the parts near its surface, the electrical fire is collected from the parts below. It is then plainly visible in the night; it appears at the stern and in the wake of every sailing vessel; every dash of an oar shews it, and every surf and spray: in storms the whole sea seems on fire. The detached particles of water then repelled from the electrified surface continually carry off the fire as it is collected; they rise and form clouds, and those clouds are highly electrified, and retain the fire till they have an opportunity of communicating it.

11. The particles of water rising in vapours, attach themselves to particles of air.

12. The particles of air are said to be hard, round, separate and distant from each other; every particle strongly repelling every other particle, whereby they recede from each other, as far as common gravity will permit.

13. The space between any three particles equally repelling each other, will be an equilateral triangle.

14. In air compressed these triangles are smaller; in rarified air they are larger.

15. Common fire joined with air increases the repulsion, enlarges the triangles, and thereby makes the air specifically lighter. Such air, among denser air, will rise.

16. Common fire, as well as electrical fire, gives repulsion to the particles of water, and destroys their attraction of cohesion; hence common fire, as well as electrical fire, assists in raising vapours.

17. Particles of water having no fire in them, mutually attract each other. Three particles of water then being attached to the three particles of a triangle of air, would by their mutual attraction operating against the air's repulsion, shorten the sides and lessen the triangle, whereby that portion of air made denser would sink to the earth with its water, and not rise to the formation of a cloud.

18. But if every particle of water attaching itself to air, brings with it a particle of common fire, the repulsion of the air being assisted and strengthened by the fire, more than obstructed by the mutual attraction of the particles of water, the triangle dilates, and that portion of air, becoming rarer and specifically lighter, rises.

19. If the particles of water bring electrical fire when they attach themselves to air, the repulsion between the particles of water electrified, joins with the natural repulsion of the air, to force its particles to a greater distance, whereby the triangles are dilated, and the air rises, carrying up with it the water.

20. If the particles of water bring with them portions of both sorts of fire, the repulsion of the particles of air is still more strengthened and increased, and the triangles farther enlarged.

21. One particle of air may be surrounded by twelve particles of water of equal size with itself, and in contact with it; and more added to those.

22. Particles of air thus loaded, would be drawn nearer together by the mutual attraction of the particles of water, did not fire, common or electrical, assist their repulsion.

23. If air thus loaded, be compressed by adverse winds, or by being driven against mountains, &c., or condensed by taking away the fire that assisted it in expanding, the triangles contract, the air with its water will descend as a dew; or, if the water surrounding one particle of air comes in contact with the water surrounding another, they coalesce and form a drop, and we have rain.

24. The sun supplies (or seems to supply) common fire to vapours, whether raised from earth or sea.

25. Those vapours which have both common and electrical fire in them, are better supported than those which have only common fire in them; for when vapours rise into the coldest region above the earth, the cold will not diminish the electrical fire, if it doth the common.

26. Hence clouds formed by vapours raised from fresh waters within land, from growing vegetables, moist earth, &c., more speedily and easily deposit their water, having but little electrical fire to repel and keep the particles separate. So that the greatest part of the water raised from the land, is let fall on the land again; and winds blowing from the land to the sea are dry, there being little use for rain on the sea, and to rob the land of its moisture in order to rain on the sea, would not appear reasonable.

27. But clouds formed by vapours raised from the sea, having both fires, and particularly a great quantity of the electrical, support their water strongly, raise it high, and being moved by winds, may bring it over the middle of the broadest continent from the middle of the widest ocean.

28. How these clouds so strongly supporting their water, are made to deposit it on the land, where it is wanted, is next to be considered.

29. If they are driven by winds against mountains, those mountains being less electrified attract them, and on contact take away their electrical fire, and being cold their common fire also; hence the particles close towards the mountains and towards each other. If the air was not much loaded, it only falls in dews on the mountain tops and sides, forms springs, and descends to the vales in rivulets, which united, make larger streams and rivers. If much loaded, the electrical fire is at once taken from the whole cloud; and, in leaving it, flashes brightly and cracks loudly; the particles instantly coalescing for want of that fire, and falling in a heavy shower.

30. When a ridge of mountains thus dams the clouds, and draws the electrical fire from the cloud first approaching it, that which next

follows, when it comes near the first cloud, now deprived of its fire, flashes into it, and begins to deposit its own water; the first cloud again flashing into the mountains; the third approaching cloud, and all succeeding ones acting in the same manner, as far back as they extend, which may be over many hundred miles of country.

31. Hence the continual storms of rain, thunder and lightning, on the east side of the Andes, which running north and south, and being vastly high, intercept all the clouds brought against them from the Atlantic ocean by the trade winds, and oblige them to deposit their waters, by which the vast rivers Amazons, La Plata, and Oroonoko are formed, which return the water in the same sea, after having fertilized a country of very great extent.

32. If a country be plain, having no mountains to intercept the electrified clouds, yet it is not without means to make them deposit their water. For if an electrified cloud coming from the sea, meets in the air a cloud raised from the land, and therefore not electrified, the first will flash its fire into the latter, and thereby both clouds shall be made suddenly to deposit water.

33. The electrified particles of the first cloud close when they lose their fire; the particles of the other clouds close in receiving it: in both they have thereby an opportunity of coalescing into drops. The concussion or jerk given to the air, contributes also to shake down the water, not only from those two clouds, but from others near them. Hence the sudden fall of rain immediately after flashes of lightning.

34. To shew this by an easy experiment: take two round pieces of pasteboard two inches diameter; from the centre and circumference of each of them suspend by fine silk threads eighteen inches long, seven small balls of wood, or seven peas equal in goodness; so will the balls appending to each pasteboard form equal equilateral triangles, one ball being in the centre, and six at equal distances from that, and from each other, and thus they represent particles of air. Dip both sets in water, and some adhering to each ball, they will represent air loaded. Dexterously electrify one set, and its balls will repel each other to a greater distance, enlarging the triangles. Could the water supported by seven balls come into contact, it would form a drop or drops so heavy as to break the cohesion it had with the balls, and so fall. Let the two sets then represent two clouds, the one a sea cloud electrified, the other a land cloud. Bring them within the sphere of attraction, and they will draw towards each other, and you will see the separated balls close thus: the first electrified ball that comes near an unelectrified ball by attraction joins it, and gives it fire; instantly they separate, and each flies to another ball of its own party, one to give, the other to receive fire; and so it proceeds through both sets, but so quick as to be in a manner instantaneous. In the cohesion they shake off and drop their water, which represents rain.

35. Thus when sea and land clouds would pass at too great a dis-

tance for the flash, they are attracted towards each other till within that distance; for the sphere of electrical attraction is far beyond the distance of flashing.

36. When a great number of clouds from the sea meet a number of clouds raised from the land, the electrical flashes appear to strike in different parts; and as the clouds are jostled and mixed by the winds, or brought near by the electrical attraction, they continue to give and receive flash after flash, till the electrical fire is equally diffused.

37. When the gun barrel (in electrical experiments), has but little electrical fire in it, you must approach it very near with your knuckle before you can draw a spark. Give it more fire, and it will give a spark at a greater distance. Two gun barrels united, and as highly electrified, will give a spark at a still greater distance. But if two gun barrels electrified will strike at two inches distance, and make a loud snap, to what a great distance may 10,000 acres of electrified clouds strike and give its fire, and how loud must be that crack?

38. It is a common thing to see clouds at different heights passing different ways, which shews different currents of air one under the other. As the air between the tropics is rarified by the sun, it rises, the denser northern and southern air pressing into its place. The air so rarified and forced up, passes northward and southward, and must descend in the polar regions, if it has no opportunity before, that the circulation may be carried on.

39. As currents of air, with the clouds therein, pass different ways, it is easy to conceive how the clouds, passing over each other, may attract each other, and so come near enough for the electrical stroke. And also how electrical clouds may be carried within land very far from the sea, before they have an opportunity to strike.

40. When the air, with its vapours raised from the ocean between the tropics, comes to descend in the polar regions, and to be in contact with the vapours arising there, the electrical fire they brought begins to be communicated, and is seen in clear nights, being first visible where it is first in motion; that is, where the contact begins, or in the most northern part; from thence the streams of light seem to shoot southerly, even up to the zenith of northern countries. But though the light seems to shoot from the north southerly, the progress of the fire is really from the south northerly, its motion beginning in the north being the reason that it is there seen first.

For the electrical fire is never visible but when in motion, and leaping from body to body, or from particle to particle through the air. When it passes through dense bodies it is unseen. When a wire makes part of the circle, in the explosion of the electrical phial, the fire, though in great quantity, passes in the wire invisibly; but in passing along a chain, it becomes visible as it leaps from link to link. In passing along leaf gilding it is visible: for the leaf gold is full of pores; hold a leaf to the light and it appears like a net, and

the fire is seen in its leaping over the vacancies. And as when a long canal filled with still water is opened at one end, in order to be discharged, the motion of the water begins first near the opened end, and proceeds towards the close end, though the water itself moves from the close towards the opened end: so the electrical fire discharged into the polar regions, perhaps from a thousand leagues length of vaporified air, appears first where it is first in motion, *i.e.*, in the most northern part, and the appearance proceeds southward, though the fire really moves northward. This is supposed to account for the Aurora Borealis.

41. When there is great heat on the land, in a particular region (the sun having shone on it perhaps several days, while the surrounding countries have been screened by clouds), the lower air is rarified and rises, the cooler denser air above descends; the clouds in that air meet from all sides, and join over the heated place; and if some are electrified, others not, lightning and thunder succeed, and showers fall. Hence thunder-gusts after heats, and cool air after gusts; the water and the clouds that bring it, coming from a higher and therefore a cooler region.

42. An electrical spark, drawn from an irregular body at some distance is scarce ever strait, but shows crooked and waving in the air. So do the flashes of lightning; the clouds being very irregular bodies.

43. As electrified clouds pass over a country, high hills and high trees, lofty towers, spires, masts of ships, chimneys, &c., as so many prominencies and points, draw the electrical fire, and the whole cloud discharges there.

44. Dangerous, therefore, is it to take shelter under a tree, during a thunder gust. It has been fatal to many, both men and beasts.

45. It is safer to be in the open field for another reason. When the clothes are wet, if a flash in its way to the ground should strike your head, it may run in the water over the surface of your body; whereas, if your clothes were dry, it would go through the body, because the blood and other humours, containing so much water, are more ready conductors.

Hence a wet rat cannot be killed by the exploding electrical bottle, when a dry rat may.

46. Common fire is in all bodies, more or less, as well as electrical fire. Perhaps they may be different modifications of the same element; or they may be different elements. The latter is by some suspected.

47. If they are different things, yet they may and do subsist together in the same body.

48. When electrical fire strikes through a body, it acts upon the common fire contained in it, and puts that fire in motion; and if there be a sufficient quantity of each kind of fire, the body will be inflamed.

49. When the quantity of common fire in the body is small, the quantity of the electrical fire (or the electrical stroke) should be

greater: if the quantity of common fire be great, less electrical fire suffices to produce the effect.

50. Thus spirits must be heated before we can fire them by the electrical spark.* If they are much heated, a small spark will do; if not the spark must be greater.

51. Till lately we could only fire warm vapours; but now we can burn hard dry rosin. And when we can procure greater electrical sparks, we may be able to fire not only unwarmed spirits, as lightning does, but even wood, by giving sufficient agitation to the common fire contained in it, as friction we know will do.

52. Sulphureous and inflammable vapours arising from the earth, are easily kindled by lightning. Besides what arise from the earth, such vapours are sent out by stacks of moist hay, corn, or other vegetables, which heat and reek. Wood rotting in old trees or buildings does the same. Such are therefore easily and often fired.

53. Metals are often melted by lightning, though perhaps not from heat in the lightning, nor altogether from agitated fire in the metals. For as whatever body can insinuate itself between the particles of metal, and overcome the attraction by which they cohere (as sundry menstrua can) will make the solid become a fluid, as well as fire, yet without heating it: so the electrical fire, or lightning, creating a violent repulsion between the particles of metal it passes through, the metal is fused.

54. If you would by a violent fire, melt off the end of a nail, which is half driven into a door, the heat given the whole nail before a part would melt, must burn the board it sticks in. And the melted part would burn the floor it dropped on. But if a sword can be melted in the scabbard, and money in a man's pocket, by lightning, without burning either, it must be cold fusion.†

55. Lightning rends some bodies. The electrical spark will strike a hole through a quire of strong paper.

56. If the source of lightning assigned in this paper be the true one, there should be little thunder heard at sea far from land. And accordingly some old sea captains of whom enquiry has been made, do affirm, that the fact agrees perfectly with the hypothesis; for that in crossing the great ocean, they seldom meet with thunder till they come into soundings; and that the islands far from the continent have very little of it. And a curious observer who lived thirteen years at Bermudas, says, there was less thunder there in that whole time than he has sometimes heard in a month at Carolina.

* We have since fired sparks without heating them, when the weather is warm. A little poured into the palm of the hand, will be warmed sufficiently by the hand, if the spirit be well rectified. Ether takes fire most readily.

† These facts, though related in several accounts, are now doubted; since it has been observed that the parts of a bell-wire which fell on the floor being broken and partly melted by lightning, did actually burn into the boards; and Mr. Kinnersley has found that a fine iron wire, melted by electricity, has had the same effect.

On the Primary and Secondary Electro-Chemical Phenomena. *By* M. Becquerel.*

By the aid of electric forces of a single pair of metals long continued, chemical effects, more or less considerable are obtained, whether the affinity of the liquid for one of the polar metals be in accordance with those forces, or operates in the contrary direction.

We every day observe that nature, having at her disposal unlimited periods of time for her operations, produces immense effects with very slender means. These means, however, frequently escape our senses, and have not been studied with care, in consequence of our attention not being directed towards them in the usual routine of our researches. It is by working on a small scale only, and close observation in every stage of the process, that chance is afforded to discover any part of the resources which nature brings into play in the production of phenomena due to molecular attraction. We will now proceed to shew some decompositions obtained by apparently feeble electric forces.

It is an established fact that voltaic action may produce chemical effects; but we are ignorant to what extent that action when very feeble, influences affinities; and whether there may not be produced at the moment these become manifest, *particular* phenomena, which disappear in the general effects when a pile of certain energy is employed.

We know, for example, that when we immerse in a metallic solution two wires of any metal whatever, which communicate with the poles of a voltaic pile, sufficiently energetic, we always obtain at the negative wire either hydrogen, liberated metal, or an oxide; but when the tension is extremely feeble do the phenomena appear in the same manner? Do all the metals possess this property in the same degree? To be enabled to resolve these questions it is necessary to diminish successively the intensity of the electricity, and at the same time to observe what passes in the decompositions. This is what we are about to do.

Place in a cylindrical glass vessel a metallic solution, a solution of copper for example, and afterwards, with great care, pour on the top of it distilled or acidulated water, so that the two liquids may not mix, but remain separate: the water being above the metallic solution. If now we immerse a plate of copper into these unmixed liquors, and permit it to remain a few hours, we find that it has become covered with precipitated copper in a metallic state. Different metallic solutions gave similar results. Hence we understand that metals may form, with their own solutions and pure water, or with water acidulated, circles of electric action sufficient to precipitate the metals.

In this case there are two electrical effects: the one resulting from the reaction of the two liquids on each other; the second from the

* Becquerel's "Traité de l' Electricité et du Magnetisme," vol. iii, p. 287.

reaction of the acidulated water on the metallic plate. It is, therefore, a compound phenomenon; for the actions conspire or antagonize accordingly as their directions are in the *same*, or in opposite directions. In the present case the two actions conspire.

Certain thermo-electrical phenomena and simple chemical actions, ordinarily disengage a sufficiency of electricity to produce decompositions resembling those we have already described. We will first direct our attention to those decompositions which are accomplished by thermo-electric currents.

Several philosophers have endeavoured, but in vain, to decompose water by the thermo-electric currents. To succeed, however, they ought to have experimented on such solutions as are easily decomposable by very feeble currents, such as the nitrate of silver and the oxide of potassium, and arranged the apparatus so as to be enabled to determine the production of the new compound.

Let us employ two wires, one of platinum the other of copper, each a few inches in length, and about a fifteenth of an inch diameter. At the end of each wire we form a small loop, by means of which they are to be linked together. The platina loop is much smaller than the copper one, which is about an eighth of an inch diameter. If we solder the two loops, the current invariably proceeds in one and the same direction, from the platinum to the copper, whether the heat be applied on the right or on the left of the point of junction. Now solder a second wire of copper to the free end of the platinum wire, after which burn a small bit of sulphur on the loop of the copper wire; this done, apply the flame of a spirit lamp to the platinum wire so as to raise it to a red heat, at the same time keeping the copper loop as cool as possible, which is best done by keeping the platinum wire in the extremity of the white part of the flame, so that it may be but at a short distance from the copper loop. If now we join the free ends of the copper wires to the ends of the wire of a galvanometer coil, we discover a current of considerable energy flowing from the platinum to the copper.

We make the copper loop much larger than the other for the purpose of keeping it less heated when the temperature of the platinum is raised to redness. If, on the other hand, we apply the focus of the heat on the side of the copper wire, the electrical effects become inverted: and if for the platinum wire we substitute one of copper, the electrical effects will still be the same. By exposing the two loops to the same degree of temperature, no electrical effects are produced.

The case of sulphur with which we cover the loop of the copper wire, enhances the electric action very sensibly.

Hence, therefore, two distinct electric effects are obtained in a closed circuit composed of two wires of different kinds of metal, accordingly as they are soldered together, or in mere contact with one another. In the first case, the current invariably proceeds in the same direction, whether we apply the heat to the right or to the left of the point of junction: in the second case it is not in the same

direction. The difference arises only from their being in one case in merely simple contact; whilst in the other there is contact in addition to chemical action which determines the formation of an oxide or a sulphuret.

The following experiment indicates the influence of chemical action in this kind of phenomena. If a piece of sulphur be burnt at one end of a copper wire which forms the coil of a galvanometer, and the other end of the wire be brought over it whilst the combustion is in full force, the current of electricity thus produced is highly energetic, and more intense than that which would be occasioned by a difference of temperature only.

We employ a tube bent into the form of a letter U, and containing a solution of the nitrate or the sulphate of copper. We immerse in the liquor in each branch a copper wire, communicating with the extremities of the wires of an apparatus like that just described. When the action has continued for an hour, the immersed wire which is connected with the negative side is covered with copper, precipitated in a metallic state. The other wire is sensibly oxidized.

Two tin wires immersed in solution of hydro-chlorate of tin, display similar results, that is to say, the wire in connection with the negative side becomes covered with crystals of tin.

Wires of zinc, of silver, and of lead, immersed in their respective solutions, exhibit similar phenomena.

Wires of platinum are without action in solutions of platinum.

We here perceive the influence of the chemical action which takes place between the wires and the solutions, on the electro-chemical decomposition.

Wires of platinum, gold, and silver, immersed in solutions of lead, tin, or copper, and prepared as those above, are equally destitute of action on them, notwithstanding the intensity of the current remains the same.

When we immerse two silver wires in a solution of nitrate and sulphate of copper, the positive wire is invariably attacked by the acid; but the negative wire shows no precipitation of the metal from the solution. In this case, therefore, the oxygen and the acid appear to be more easily transported to the positive pole than the copper to the negative pole.

In a solution of the nitrate of silver, platinum wires occasion precipitation of silver as decidedly as by silver wires; but the silver wires occasion a more copious precipitation than is accomplished by the platinum. The difference in the action of these two metals is rendered very obvious by immersing in the solution at the same time, a silver wire rolled round a platinum wire.

We thus see that by feeble currents of uniform intensity, the easily reduced metals are more disposed to be precipitated upon metals of their own kind, than upon any other, in all those cases where the immersed metal itself does not occasion precipitation, in the manner that iron precipitates copper from its solution. This remarkable fact cannot be observed only by the employment of an

electrical apparatus of very feeble tension, for when the electric tension amounts to a certain degree, the metal liberated from its solution, proceeds to the negative terminal metal, of whatever kind it may be.

To what cause, under these circumstances, are we to attribute this predilection of a metal, combined with an acid, for a plate of metal of its own kind? The force of cohesion, whatever that may be, is the only influence conceivable: because that force must be supposed to act with greater energy on similar than on dissimilar molecules. In this case the force of aggregation added to that of the electric current, ought to determine the precipitation. But we must not overlook the chemical action of the solution on the positive metal, which is highly conducive to the general effect.

If it be required to obtain continuous effects with the thermo-electric apparatus, the copper loop into which the platina loop is linked, must be renewed from time to time, because the former becomes completely oxidized by a continuation of the process, and the continuity thus becomes broken, and the electro-chemical action consequently ceases.

An apparatus formed of a platinum wire and a wire of iron has no action sufficient to produce decomposition. This negative effect is undoubtedly owing to the peculiar electrical properties due to iron: of which we have already spoken and now return to again.

The apparatus about to be described is destined to produce slow continuous electric actions.

We take two small glass jars, into one of them we place some nitric acid, and in the other some potash dissolved in water: and establish a communication between them by means of a bent glass tube filled with potters' clay moistened with a solution of nitrate of of potash, or of common salt. We then place in each vessel a plate of platinum to which is attached a wire of the same metal. At the free end of each platinum wire, we fix the end of the wire on which the experiment is to be made. The plate in contact with the alkali receives the negative electricity which becomes disengaged by its reaction on the water, or the solution of the nitrate or the chloride, and the plate in the acid receives the positive electricity which is liberated during the same reaction. We have thus a permanent pile, by taking care to close the vessels so as to prevent evaporation and the action of the air on the alkali. By immersing several plates of platinum in the liquids in the jars we may put several sets of apparatus into action at the same time.

We may employ water as a substitute for the potash, and a copper wire in each vessel. Even with this arrangement we obtain chemical action, and an electric current, from the copper to the acid, of sufficient energy to produce decompositions similar to those already described.

It will not be doubted that in electro-chemical decomposition, produced by electro currents of low tension, the oxygen and acids assemble at the positive pole, as decidedly as in those energetic

currents proceeding from the action of several voltaic elements. The wire attached to the negative pole is always seen to be covered with precipitated metal: but the determination of oxygen and acid at the positive polar wire, cannot always be detected. In such cases they have formed an insoluble compound.

We have also shown above that, when two silver wires are immersed in a solution of nitrate of copper, and in connexion with the voltaic apparatus, the positive wire becomes sensibly altered, whilst the negative wire retains its metallic brilliancy, although no trace of metallic copper can be detected on its surface. We may have the choice of two modes of explanation in this circumstance. We may suppose the copper to have been arrested in the solution, as is sometimes the case when an insoluble compound is formed; and in the present case no transfer of the elements of the nitrate has taken place, although it has been decomposed; or we may suppose the deposit on the negative wire was too slight to be observed.

We may now employ two small cylindrical glass vessels, in one of which is placed a solution of the nitrate of barytes, and in the other a solution of the sulphate of copper; and form a communication between the solutions by means of a bent tube filled with potters' clay, well soaked with a solution of common salt, which will facilitate the transfer of electricity between the solutions. Into the sulphate solution we immerse a copper wire connected with the negative side of the apparatus; and in the nitrate of barytes the other wire. If now, the sulphuric acid proceeds towards the positive pole it will enter the solution of barytes, and by uniting with that base will form a precipitate.

Now, this is what actually happens. After the experiment has been continued four or five hours, the negative end is covered with copper: the solution of nitrate of barytes is not sensibly disturbed, and the positive end is oxidized. Are we to infer, from this circumstance, that nothing but the oxygen has been transferred, and that the sulphuric acid has remained in the sulphate? This question can be answered only by an analysis of the secondary productions formed in the tube: and there is reason to believe that sulphate of barytes would be found. In general when the production is not found at one of the poles, we may be certain that it has been arrested on its way by superior affinities.

The acetates and sub-acetates of lead are also decomposed by means of leaden wires; but the acetate of copper, and the saturated solution of the same salt in ammonia resist an electric action of low tension, when copper wires are immersed in these solutions. These remarks are of some importance because these salts decompose with great facility by ordinary chemical processes.

Experiments and Observations made with the View of Ascertaining the Nature of the Gas produced by passing Electric Discharges through Water; with a Description of the Apparatus for these Experiments. By George Pearson, M.D., F.R.S.*

In the "Journal de Physique" for the month of November, 1789, were published the very curious and interesting experiments of Messrs. Paets Van Troostwyk, and Deiman, with the assistance of Mr. Cuthbertson, on the apparent decomposition of water by electric discharges.

The apparatus employed was a tube twelve inches in length, and its bore was one-eighth of an inch in diameter, English measure, which was hermetically sealed at one end, and while it was sealing an inch and a half of gold or platina wire was introduced within the tube, and fixed into the closed end by melting the glass around the extremity of the wire. Another wire of platina or of gold, with platina wire at its extremity, immersed in quicksilver, was introduced at the open end of the tube, which extended to within five-eighths of an inch of the upper wire, which, as was just said, was fixed into the sealed extremity.†

The tube was filled with distilled water, which had been freed from air by means of Cuthbertson's last improved air pump, of the greatest rarifying power. As the open end of the tube was immersed in a cup of quicksilver, a little common air was let up into the convex part of the curved end of the tube, with the view of preventing fracture from the electrical discharges.

The wire which passed through the sealed extremity was set in contact with a brass insulated ball; and this insulated ball was placed at a little distance from the prime conductor of the electrical machine. The wire of the lower or open extremity, immersed in quicksilver, communicated by a wire or chain with the exterior coated surface of a Leyden jar, which contained about a square foot of coating; and the ball of the jar was in contact with the prime conductor.

The electrical machine consisted of two plates of thirty-one inches in diameter, and similar to that of Teyler. It possessed the power of causing the jar to discharge itself twenty-five times in fifteen revolutions. When the brass ball and that of the prime conductor were

* From Nicholson's Journal.

† In another part of Mr. Van Troostwyk's memoir it is stated that the distance was an inch and a quarter from the end of the upper wire to the top of the lower wire; and that the distance between the insulated ball and the prime conductor, was at first three-fourths of an inch, but that afterwards it was increased to one inch. Although the wire fastened into the top of the tube was said to be an inch and a half in length, it is observed that when a column of three-eighths of an inch of air was collected, it was almost at the extremity of the upper wire. From these and other inaccuracies, it will be made appear that no one, from the account published, has been able to repeat the experiment.

in contact, no air or gas was disengaged from the water by the electrical discharges; but on gradually increasing their distance from one another, the position was found in which gas was disengaged, and which ascended immediately to the top of the tube. By continuing the discharges, gas continued to be disengaged and ascend, till it reached nearly to the lower extremity of the upper wire; and then a discharge occasioned the whole of the gas to disappear, a small portion excepted, and its place was consequently supplied by water.

The residuary portion of gas being let out after each experiment, and the discharges being continued in the same water, this residuary gas was left in smaller and smaller quantity; so that after four experiments, probably made on the same day, it did not amount to more than 1-80th of the bulk of gas which had been produced. If it had been possible to pass electric sparks through this very small quantity of gas a second time, or oftener, it was supposed it would have been diminished still more. But when the tube had been left for a night only filled with water, the residuary gas was in greater quantity than after the last experiment the preceding day.*

It was concluded that the gas produced by the electrical discharges was oxygen and hydrogen gas, from decompounded water:—

1. Because no other gas hitherto known instantly disappears on passing through it an electric spark.

2. The gas obtained must have been the oxygen and hydrogen of decompounded water, because they were in exactly those proportions in which by combination they reproduce water; the trifling residue being considered to be merely a portion of air which had been dissolved in the water.

3. Liquids which are not compounded of hydrogen and oxygen, as sulphuric and nitric acids, afforded gas by the electric discharges, but which did not disappear on passing through it an electric spark; but which did disappear on adding to it nitrous gas over water. Mr. Schurer also asserts, on the authority of Mr. Van Troostwyk, that even liquid muriatic acid, which contains a very large proportion of water, affords hydrogen gas only, the oxygen being absorbed by the muriatic acid, and becoming oxy-muriatic acid.

From much experience I can safely affirm, that it is scarcely possible for the student, or even the proficient, to institute the above experiment with success from the explanation published. Hence, during the six years which have elapsed since its publication, no confirmation has been published except the experiment repeated by Mr. Cuthbertson for my satisfaction, as related in my work on the

* In at least fifty experiments I have never seen the residue of gas less than 1-40th of the gas produced, although the water had been freed from air by the most effectual means. But Mr. Schurer (Annales de Chimie, tom. v, p. 276), testifies that he saw Mr. Van Troostwyk make the experiment, and that after it was repeated many times on the same parcel of water, there was no residue at all. I have very good grounds for believing that this is one of the number of inaccuracies in the account published of this subject.

chemical nomenclature; but I have heard of many persons, and some of whom were experienced electricians and chemists, who made the attempt.

Since Mr. Cuthbertson came to reside in London, I have learned from him the circumstances requisite to the success of the experiment; and I have received from him also very great assistance in continuing a process with the objects I had in view, the tediousness and even difficulties of which can only be conceived by those who have been engaged in the same pursuit.

I am very sensible that it would be unnecessary for me to explain the importance of a process which may at last afford demonstration of the composition of water, by the fullest and unequivocal evidence of its analysis and synthesis; a demonstration which no other single process but the present promises to afford.

I propose therefore in this paper:

1. To give such a description of the experiment of rendering water into gas by electric discharges, as shall enable any person who is versed in pneumatic chemistry, and acquainted with the theory and practice of electricity, to repeat it with success. By this description, also, I apprehend I shall make known more generally the very elegant, and frequently most satisfactory mode of decompounding and compounding bodies, by means of the fire of the electric discharge.

2. It is proposed to relate the additional evidence which I have already obtained from this process, concerning the composition of water. For although it seems most probable that water is decompounded in Mr. Van Troostwyk's experiment, it must be confessed that it does not make appear a single unequivocal and decisive property of hydrogen and oxygen in the gas produced. The disappearance of this gas by combustion, or in some other way, instantly on passing through it an electric spark, it is true, is a property known only to belong to the mixture of oxygen and hydrogen gas; but it is well ascertained, that things of totally different species may agree in one or more properties. And there is at least a possibility, that electric discharges may produce various other kinds of gases, in water, beside hydrogen and oxygen from decompounded water; and which may have the property of instantly disappearing on passing through them an electric spark.

3. I shall attempt to resolve the phenomena of the process into a general law of the action of fire, or of the joint action of caloric and light.

SECTION I.

Of the Manner of Conducting the Process.

Electric discharges may be employed in two different manners to decompound water. One of these is by what has been termed the interrupted explosion; which was the method, although not so explained, of Mr. Van Troostwyk. And the other method is by means

of the uninterrupted or complete explosion; for which there are two different kinds of apparatus. These were invented by Mr. Cuthbertson in the course of my investigation of this subject.

To succeed by the method of the interrupted explosion, the following are the necessary parts of the apparatus to be used, and the circumstances to be attended to :—

1. *The electrical machine must possess sufficient power.*—I do not think any cylindrical machine can be made to answer in this process if a large quantity of gas be required; because they cannot be made to act with due regularity, constancy and force. Inequality of the surface of the cylinder is unavoidable, which causes undulation. A cylindrical machine never continues in full force above five or six minutes, without fresh amalgam. Hence, from the repeated amalgamization, the discharges will be so variable that the tubes must be frequently broken.

I used plate machines of a peculiar construction by Mr. Cuthbertson. These machines do not require fresh amalgamization oftener than once in eight hours, and they possess superior powers of acting, in point of regularity, force, and duration. A plate of twenty-four inches in diameter, with a jar containing one hundred and fifty square inches of coating, afforded an adequate discharge every second or third revolution, for several hours; and for a still longer time every third or fourth revolution, with one application of amalgam. A thirty-one inch plate machine afforded a due discharge at first every revolution, and afterwards every second revolution for many hours, with one application of amalgam. The most useful and expeditious machine was that with two plates, each twenty-four inches in diameter, and similar to that of Teyler. It produced twenty-five discharges every fifteen revolutions for an hour or two; and for four or five hours longer a discharge was produced by less than two revolutions, with one amalgamization.

2. *The Leyden jar must have a sufficient quantity of coated surface;* without which the discharge will not be sufficiently powerful to produce the gas required. The proper quantity, as found by experience, was about one hundred and fifty or one hundred and sixty square inches, with an usual proportional prime conductor.

3. *The distance between the insulated ball, and the prime conductor, must always be less than the distance between the extremities of the wires.*—Not the least notice of this circumstance has been taken; yet without attention to it the experiment can never succeed, or only for a very short time. Accordingly as the distance between the extremities of the wires within the tube answered best when it was 5-8ths or 7-8ths of an inch, the distance between the insulated ball and prime conductor was seldom more, but frequently less, than 5-8ths or 6-8ths of an inch. The eye must be kept upon the sparks within the tube, and by practice a person may become a judge of their force by their vividness; which will direct him to bring the receiving ball nearer to the prime conductor, when there appears

danger of the tube being broken; and on the contrary, to remove them to a greater distance from one another when the sparks do not produce gas duly from the water. When the discharge is of the most productive force, both ends of the wire within the tube will be illuminated by a spark; but, when it is weaker, one end only of the wire will be illuminated; and when this is the case, there is no risk of the tube being fractured, but gas will rise from the end of one wire only instead of two.

4. *The extremities of the upper and under wire within the tube must be at a certain distance from one another.*—If they be too near one another, the points of them will not be illuminated; and provided the insulated ball be as near to the prime conductor as the two wires are to one another, the tube will be broken, because there will be a complete explosion. But if the wires be at too great a distance from one another, the electric fluid of the discharge will be so diffused through the water that no gas will be produced. If the Leyden jar contain, as above stated, 150 or 160 square inches of coated surface; and the ball of the prime conductor and of the insulated ball be about three inches in diameter, the distance between the wires which generally answered best, was about 5-8ths or 7-8ths of an inch, as above said. The narrower the bore of the tube, the greater may be the distance between the two wires; accordingly, the distance may be one inch with a tube 1-14th of an inch wide.

5. *The upper wire fixed into the closed extremity of the tubes must be of a proper length and thickness.*—If this wire be too long, either the discharge will not be carried through to the end of the lower wire in sufficient quantity to produce gas; or, if it be in sufficient quantity to produce gas, the tube will be fractured. The smaller the diameter of the tube, the longer may be the upper wire for a reason to be given under the next head. I generally found that the discharge requisite to produce gas fractured the tube if the upper wire was more than 6-8ths or 7-8ths of an inch in length, within a tube of more than 1-8th of an inch in diameter. But with very narrow tubes, such as those of 1-16th of an inch in diameter, I frequently succeeded when the upper wire was 1¼ of an inch in length. It is obvious, that the shorter the upper wire the more readily will gas be produced: the process, however, will be rendered still more tedious in those cases in which a quantity of gas is to be collected in a reservoir for examination; on account of the time consumed in transferring such small parcels of gas by each experiment.

The diameter of the upper wire cannot, perhaps, be too small; for the greater its superficies, the more electric fluid will be parted with to the surrounding water. Hence platina wire of the finest sort, as that 1-240th of an inch in diameter, may be used with superior advantages. This sort of wire also cannot be melted while it is soldering to the glass, which can hardly be prevented with fine wire of other metals. However, I found that copper, brass, or gilt wire, of about 1-80 or 1-100th of an inch in diameter, could be soldered to the glass, and answered perfectly. I did not find that any of the

metal wires were affected by the discharge; but iron or steel ones are not proper, on account of their being so soon oxidized by the water, and consequently extricating from it hydrogen gas. I did not find any advantage from using several small wires twisted together, but separated at the end within the tube; for gas was extricated generally at the point of one of them only, namely the undermost. I think care should be taken to fix the upper wire so that it be in the middle of the tube, as in that case the tube will be less liable to be broken. As to the under wire, the diameter of it seems to be of little importance; for, if it be a thick one, as much gas will be extricated as if it were a small one; because the electric discharge will take the first point of the surface of the wire where the gas is produced.

6. *The tubes must be of a proper length and diameter.*—If they be shorter than seven inches, the discharge will be liable to pass over the outside; and if they be longer than twelve inches, they will be of an inconvenient length. I found the most convenient length to be from nine to ten inches, exclusive of the curved part. The curved part was found very useful in preventing air ascending, which was accidentally let into the tube, by which the product of gas from the experiment would have been contaminated. Such curved extremities were however less convenient than straight ones, on account of the greater difficulty of transferring from the former than from the latter.

The diameter of the tubes should not be more than 1-8th or less than 1-12th of an inch. At least in my experiments these tubes answered best. If they were wider, the discharge requisite to produce gas broke the tube. If the tubes be narrower than just mentioned, the experimenter will find it difficult and tedious to transfer the gas through the curved part into a reservoir, in those cases in which a large quantity is wanted for examination. Where however the object is merely to show the production of gas by means of the electrical discharge passed through water between two wires, and the instant disappearance of gas so produced by passing through it an electric spark, the narrowest tubes are most eligible, as with them the experiment can be made in a shorter time. But there was one inconvenience experienced from very narrow tubes, namely, the bubbles of gas were very apt to hang near the end of the upper wire instead of ascending; and they were apt to form large bubbles with water between each; in which situation the discharge frequently fractured the tube, or made gas to disappear by combustion in the course of the experiment. To possess at once the advantages of the narrow and wide tubes, about three to six inches of narrow tube were joined by fusion to a bottom part of a wider tube; into the curve of which tube the gas produced was let up from time to time; so that this part frequently contained the products of ten or more experiments before the gas was transferred into the reservoir. The vast number of tubes which were broken in this experiment induced me to try various different kinds of them. I experienced no advan-

tages from annealing tubes; but their being seemingly rendered more brittle and harder by this treatment was an effect which I least of all expected. Tubes with thin sides answered just as well as with thick ones. Bohemian green glass tubes were found excellent for this experiment, as they were much less apt to be cracked by the discharges than any kind of English glass. To save time and trouble in so frequently letting out gas produced in very narrow tubes, a small bulb was made at the sealed end. In this case, however, as the upper wire must be shorter than other narrow tubes, it was more difficult to regulate the explosion.

Although common atmospherical air is an electric, and water is a conductor of electricity, it appears that the discharge passes with more resistance from wire to wire through water in the above experiment, than through common air under otherwise the same circumstances. The reason of which is this: air being an elastic and very rare fluid, it more readily gives way to the electric discharge than water; and it can therefore pass through a longer and thicker column of air between two wires, without breaking the glass tubes, than it can through water. For although water is a conductor, yet in a very small quantity it is a very indifferent one; so that its density and defect of elasticity more than compensate for its conducting power. Hence also, and on account of the conducting power of water, the reason of the upper wire in this experiment being shorter in proportion as the tube is wider; and on the same account will be seen the reason of the advantages of a small upper wire over thicker ones.

It will be necessary to add, that the tubes with curved extremities can only be filled by setting them in water under a receiver, and exhausting the air from the receiver, tubes, and water; then, by letting in the air again, the water will be forced up into the tubes. Sometimes, however, I have filled the tubes by setting them in Papin's digester.

These are the directions for making the experiment; but the rationale of it cannot be understood unless the nature of the interrupted explosion be explained; because I believe books on electricity do not contain the necessary information. It must be considered, in the above experiment, that if in place of water the tubes be filled with air, the whole of the charge of the Leyden jar will pass, at each explosion, from the upper to the under wire, and no interruption in the discharge will happen; but if they are filled with water, then an interrupted discharge may be caused; by which is meant that a part of the charge only passes at each explosion through the water, from wire to wire, and with much diminished velocity. The residuary electricity in the Leyden jar is nearly one half; as may be accurately demonstrated from the difference in point of density, elasticity, and conducting power of the medium of water and air, as already observed. It must be added, that although water in large quantity is a good conductor, and air is not, yet water being here in very small quantity, it proves a

bad conductor, as is the case with the very best conductors. A cubic foot of water is only just capable of receiving, or letting pass through it, a full discharge from a jar of one foot of coated surface; and the quantity of water employed in this experiment not being $\frac{1}{100000}$th part of a cubic foot, it is a very imperfect conductor; so that a interrupted discharge only can pass through the tube, without dispersing the whole of the water. But if the discharge be not seemingly as strong as the tube can bear without breaking, the gas is not produced from it; and on this point hinges this extremely delicate process.

To succeed by the method of the complete or uninterrupted explosion, the following apparatus must be used, and rules observed:—

1. A tube is employed about four or five inches in length, and 1-5th or 1-6th of an inch in diameter. One end is mounted with a brass cap, and the other end is sealed at the lamp, with a wire about 1-40th of an inch in thickness fixed into it, as above described; which extends into the brass cap, so as to be almost in contact when the explosion is made. If the wire touches the brass cap there will be no explosion. The tube being filled with, and set in, a cup of water, the discharge may be made into it as in the above described process; but here the insulated ball must be placed at a greater distance from the prime conductor, and a Leyden jar with only fifty square inches of coating, will answer the purpose. In this way of making the experiment gas is produced by each discharge in the brass tube, in much greater quantity, with much less frequent accidents, and less trouble, than in the former method with the interrupted discharge. But the gas obtained with this apparatus always contains a larger proportion of atmospherical air, on account of the quantity of water, and more immediate and extensive communication of it with the atmosphere. By repeated discharges there is an impression made in the brass tube, in the part where the discharge passes through it, and at last a small hole is made in that part. On this account the same mounted tube cannot serve for producing a large quantity of gas.

The other sort of apparatus invented by Mr. Cuthbertson consisted of a glass tube half an inch wide and about five inches in length, mounted at one end with a brass funnel, and inverted in a brass dish; but afterwards the tube was blown funnel-wise at the end. The other end must have a wire about 1-40th of an inch thick, sealed into it at the lamp; which wire extends to nearly the bottom of the brass dish in which the tube stands.

The exact distance between the end of the wire and brass dish must be found by trials; that which generally answered in my experiments was about 1-20th of an inch. If it be properly arranged, gas will be produced at each discharge.

The Leyden jar used with this apparatus must contain about 150 square inches of coating.

The distance between the insulated ball and the prime conductor, at which the experiment succeeded, was commonly about half an inch.

If experiments be proposed in which electrical discharges must be passed through water or other fluids, for even a much longer time than was consumed in performing those referred to or related in this paper, it may be an object to employ the wind, or perhaps the power of a horse, to turn the electrical machines, the expense of labourers being considerable.

SECTION II.

From my journal of the numerous experiments made during the course of nearly two years, I shall select those which will serve to explain the nature of the process, and shew the power of the plate electrical machines; and I shall particularly relate those experiments which afforded the most useful results concerning the nature of the gas obtained.

1. *With interrupted Discharges.*

Experiment A. About 1600 of these discharges by means of a 34-inch single plate electrical machine, in nearly three hours, produced from New River water taken from the cistern, and which had not been freed from air by the air pump or boiling, a column of gas, 2-3rds of an inch in length, and 1-9th of an inch wide. On passing through this gas, between the two wires of the tube in which it was produced, a single electrical spark, its bulk was instantly diminished to 2-3rds. In other experiments the bulk of gas was only diminished to about one half; and the result was the same with distilled water.

B. The experiment A being repeated several times with distilled and New River water, freed from air by the air pump or long boiling, the quantity of gas just mentioned was obtained in about four hours.

On passing an electric spark through this gas in the situation above mentioned, its bulk was instantly diminished, in some cases 15-16ths, and in others 19-20ths.

C. 1600 interrupted discharges, by means of a 32-inch plate machine, produced from New River water and distilled water, freed from their air by the air pump, a column of gas about 3-4ths of an inch in length, and 1-9th of an inch in diameter, in the space of three hours. It was reduced in bulk 19-20ths by passing through it a single electrical spark.

D. 500 revolutions of the 32-inch plate machine, in three quarters of an hour, produced 600 interrupted discharges in river water freed from air by the air pump, by which a column of gas, half an inch in length and 1-10th of an inch in diameter, was obtained. It was diminished as usual by an electric spark 19-20ths of its bulk.

E. Nearly four days' incessant labour with the 32-inch machine, produced only 56,5488 cubes of gas, of 1-10th of an inch each, on account of the usual accidents during the process. The air had been exhausted by setting the water under the receiver of the air pump.

F. It was found that 6000 interrupted discharges produced about three inches in length of gas, measured in a tube 3-20ths of an inch in width, from water out of which its air had been drawn by the air pump.

G. It appeared from many experiments, that the same unboiled water, or water from which the air had not been exhausted by the air pump, which had repeatedly yielded gas by passing through it electrical discharges, always left a residue of gas which the electrical spark did not diminish; and this residue was in nearly the same quantity after six or seven experiments, each of which afforded a column of gas half an inch in length and 1-9th of an inch in diameter, as was left on passing the electric spark through the gas afforded by the third or fourth experiment.

Hence it seems that water is discompounded by the electric discharge, before the whole of the common or atmospherical air is detached from the water by merely the impulse of each discharge. Yet I think it probable that, after the discharges have been passed through the same water for a certain time, the whole of the air contained in water will be expelled, and no gas be produced, but that compounded by means of the electric fire from water; in which case, supposing the gas so produced to be at least merely hydrogen and oxygen gas, it will totally disappear on passing through it an electrical spark. But I have never been able to determine this point, because the tubes were always broken after obtaining a few products, or long before it could reasonably be supposed the whole of the air of the water was expelled from it.

H. To the gas obtained in the experiment E was added, over water, an equal bulk of almost pure nitrous gas. Fumes of nitrous acid appeared, and the gas examined was reduced almost 1-3rd of its bulk. A small bubble more of nitrous gas being let up, no further diminution took place. To this residue was added half its bulk of oxygen gas, obtained from oxy-muriate of potash.

This mixture of gases having stood several days over well burnt lime and boiled quicksilver, an electric spark was passed through the mixture over quicksilver, by which its bulk was instantly diminished 1-4th; but no moisture could be perceived upon the sides of the tube or on the quicksilver. The failure of the appearance of moisture was imputed to a bit of lime accidentally left in the tube which was burst by the explosion, and dispersed through the tube; or else the quantity of water produced was so small, comparatively with the residuary gas, that the water was dissolved by it in the moment of its composition. For, supposing water to have been compounded, it could not amount to the 1-100th part of a grain, and the residuary gas was at least 2,000 times this bulk.

That a quantity of water can be compounded under the same circumstances as in this experiment, and be apparently dissolved in air, so as to escape observation, even with a lens, was proved by passing an electric spark through a mixture of hydrogen and oxygen gas, well dried by standing over lime.

2. *With complete or uninterrupted Discharges.*

The gas obtained by the first described kind of apparatus for the uninterrupted discharges, always left a residue of at least one-fourth of its bulk, on passing through it the electric spark, even when water was used which had been freed from air by boiling, or the air pump. Nor will this result appear surprising, when it is considered how liable the water in this apparatus is to mix and absorb air during the experiment. However, this method would have been extremely valuable, if the next other method had not been discovered; for gas may be obtained by it with fewer accidents, and much more rapidly, than with the interrupted discharges. The apparatus is also much more easily fitted-up, and is more simple. But I think it unnecessary to particularly relate any experiments, as they afforded the same results as those already described, and as those next to be related.

The following experiments were made with the apparatus :—

Experiment 1. At 0h. 40′ P.M., began to produce discharges with a double plate 24-inch machine, in water taken from the cistern; and at 12h. 6′ P.M., of the same day, there had been written down 10,200 discharges, each of which occasioned air to ascend from the bottom of the wire and brass cup. The quantity of air obtained was now apparently about one-fourth of a cubical inch, and it occupied nearly half of the tube, the water in which was by this time very muddy.

After standing till the day following at noon, when the process was again commenced, it did not appear that any of the gas had been absorbed by the water over which it stood.

At 2h. 35′ P.M., began to produce discharges, and at 8h. P. M., had passed 6,636; which, together with those of the preceding day, amounted to 16,836. The tube was now 5-8ths full of gas, and there seemed to be almost half a cubical inch; for it was observed that the gas was this day yielded at double the rate it had been the day before. This was accounted for from the diminished pressure upon the electric fire, by the tube containing gas instead of water.

At this time, namely, at 8h. P.M., I was surprised on the passing of a discharge by a vivid illumination of the whole tube, and a violent commotion within it, with, at the same time, the rushing up of water, instantly to occupy rather more than 5-8ths of the space which had been occupied by gas.

The residue of gas was not diminished further by an electric spark; and to the test of nitrous gas it appeared to be rather worse than atmospherical air, as it consisted of rather less than one part of oxygen, and three parts of nitrogen or azotic gas.

It seemed as if the electrical discharge had kindled the oxygen and hydrogen gas of the decompounded gas, by flying from the bottom of the wire to the brass funnel, so that the fire returned into the tube where it passed through the gas. Or the combustion

might be occasioned by a chain of bubbles reaching from the brass dish to the surface of the water in the tube, which was set on fire in its ascent, and thus produced combustion of the whole of the gas of decompounded water.

That this phenomena was from the combustion here supposed, was in some degree proved, by finding that the mixture of hydrogen gas and atmospherical air, under the same circumstances, was kindled in the same manner.

Exp. 2. With a double plate electrical machine, 24-inches in diameter, and a similar apparatus to that in the last experiment, 14,600 discharges produced at least one-third of a cubical inch of gas. While I was measuring with a pair of compasses the quantity of gas produced, the points of them being in contact with part of the tube occupied by gas, I was again surprised on the passing of a discharge by an illumination of the whole tube, and the rushing up, with considerable commotion, of water, to occupy about two-thirds of the space filled with gas.

The residuary air was found, as in the former experiment, to be rather worse than atmospherical air.

It was concluded that the points of the compasses had attracted electrical fire from the wire to the sides of the glass, and thereby kindled the hydrogen and oxygen gas of decompounded water. But to determine this question, I introduced into the same tube a mixture of one measure of oxygen and two measures of hydrogen gas, to occupy nearly the same space in the tube as the gas had occupied, then passing an electrical discharge through it, no combustion was excited; but on passing a discharge while the compasses were in contact with the tube, as just mentioned, an illumination and violent commotion were produced, with the rushing up of water, to leave only 1-8th of the gas as a residue. On repeating this experiment with two measures of atmospherical air, and one of hydrogen gas, combustion could not be excited; nor with one measure of atmospherical air, and two of hydrogen; but on adding to this last mixture one measure of oxygen gas, the electrical discharge produced the phenomena of combustion just mentioned, with the rushing up of water, to occupy about two-thirds of the space which was occupied by the gases.

Exp. 3. Having passed 12,000 discharges through water, with the apparatus of the preceding experiment, and thereby obtained only one-fifth of a cubical inch of gas; and having observed that the quantity of gas was not greater than when only 8,000 discharges had been passed, and yet bubbles had been seen to be produced on each discharge, as copiously, or more so, by the last 3,000 or 4,000 discharges, as before, I began to suspect that part of the gas had been destroyed during the process, or had been absorbed. While I was considering how to account for this disappearance of gas, and was at the same time looking at the tube through which the discharges were passing, I observed one of them to be attended

with a diminution, instantly, of about one-fifth of the gas produced, and with a slight explosion. I was now sure, from this phenomenon, and from the unequal augmentation of the bulk of the gas at given times during the process, that combustion had been excited several times before, not only in the present experiment, but perhaps in the former ones, without observing it. I conceived that a gradual combustion also very probably took place in this process by the kindling of bubbles of gas in their ascent through the water. I now perceived that the discharges ought to be produced more slowly, or the tubes to be wider, to allow the bubbles to pass quite through the water, in order to avoid the ascension of gas during the process. My calculation, also, that 35,000 to 40,000 discharges were requisite to produce one cubical inch of gas from water containing its usual quantity of common air, was rendered much more vague by this ascension, so often liable to be occasioned.

To the gas which remained in the tube in this experiment was added an equal bulk of nitrous gas; the mixture diminished to 1·5; and on adding to the residue half its bulk of oxygen gas, and passing through it the electrical spark, no accession or diminution of bulk was produced. Hence all the hydrogen gas and oxygen gas, produced by the decomposition of the water, had been burnt during the process; the oxygen gas thus detected being considered to be only that expelled from the water.

Exp. 4. By means of electrical discharges with the apparatus used in the preceding experiment, I obtained gas from New River water, letting it up into a reservoir, as soon as about 1-20th of a cubic inch was produced, till I had collected 1-8th of a cubic inch. To this was added an equal bulk of nitrous gas on which the mixture diminished to 1·2; and on the addition of a little more nitrous gas, no further diminution took place. To this residue half its bulk of oxygen was added; and this mixture of gases being well dried, by standing over lime and boiled quicksilver, an electric spark was passed through it, by which a diminution of 1-6th of its bulk took place. A little dew was then seen upon the sides of the tube where the quicksilver had risen; and with the aid of a lens the same appearance was perceived on the part of the tube containing the residue of gas.

It may now be expected, that I should have made the experiment with this apparatus on distilled water, freed from its air, not only by long boiling or the air pump, but by sending through it several hundred electrical discharges. It would also have been, to some persons, more satisfactory if the experiment had been made upon a larger scale, so as to have produced the combustion of a much larger quantity of gas, and consequently have produced a greater quantity of water. As, however, I apprehend, the experiments contained in this paper, when well considered by competent judges, will be found to explain the nature of the gas procured from water by electric discharges, and as another very important subject demands my

attention, the honour of more splendid and convincing experiments must be reserved for other enquirers. If the same sacrifices be made by them, as have been made in performing the present experiments, I think it is scarcely possible but that still further light concerning the composition of water should be procured, as well as concerning oils, alcohol, acids, &c.: to the investigation of the composition of which, the mode of analysis and synthesis here indicated may be applied.

SECTION III.

On the Mode of Action of Electric Discharges.

The mere concussion by the electric discharges, appears to extricate not only the air dissolved in water, which can be separated from it by boiling and the air pump, but also that which remains in water, notwithstanding these means of extricating it have been employed.

The quantity of this air varies in the same, and different waters, according to circumstances. New River water from the cistern yielded 1-5th of its bulk of air, when placed by Mr. Cuthbertson under the receiver of his most powerful air pump; but in the same same situation, New River water taken from a tub exposed to the atmosphere for some time yielded its own bulk of air. Hence the gas procured by the first one, two, or even three hundred explosions in water, containing its natural quantity of air, is diminished very little by an electric spark.

The gas thus separable from water, like atmospherical air, consists of oxygen and nitrogen, or azotic gas; which may be in exactly the same proportions as in atmospherical air; for the water may retain one kind of gas more tenaciously than the other; and on this account the air separated may be better or worse than atmospherical air at different peiods of the process for extricating it.

With regard to the gas which instantly disappears on passing through it an electric spark, its nature is shewn by *(a)* this very property of thus diminishing, and by the following properties:—

(b) A certain quantity of nitrous gas instantly disappeared, apparently composing nitrous acid by being added to the gas *(a)* Exp. IV.

Oxygen gas being added to the residue, after saturation with nitrous gas, and an electric spark being applied to the mixture of gases, well dried, a considerable diminution immediately took place, and water was produced.

(c) Combustion from hydrogen and oxygen gas took place when the tube was about three-fourths full of gas, Exp. I; which was confirmed by passing an electric discharge, under the same circumstances, through a mixture of hydrogen and oxygen gas, Exp. II.

(d) Combustion from hydrogen and oxygen gas took place when the points of the compasses were accidentally applied to the part of the tube containing gas; which was confirmed by passing a discharge, under the same circumstances, through a mixture of

hydrogen gas, while the points of the compasses were applied to the tube.

(e) The observations made of the kindling of gas, in small quantities, from time to time, during the process of obtaining it, particularly while it was ascending in chains of bubbles, or was adhering to the funnel of the tube, Exp. III, confirm the evidence in favour of this gas being hydrogen and oxygen gas.

The evidence contained under the above heads *(a)—(e)* considered singly and conjunctively, I apprehend, must be admitted by the most rigorous reasoner, or severest logician, to be demonstrative that hydrogen and oxygen gas were produced by passing electric discharges through water.

With regard to the origin and mode of production of these two gases, our present observations and experiments do not afford complete demonstrative evidence; but although some hypothesis must be admitted, I conceive that the body of evidence we possess can afford a satisfactory interpretation of the phenomena.

It is demonstrable that the electric discharge and spark contain fire; and very probably they are merely a state of fire. Fire may be considered as consisting of caloric and light; but it is at least as consistent with the phenomena, and it is more philosophical, because it is more simple, to consider light not as a distinct species of matter, but as a state of caloric, which is manifested by its producing the sensation termed vision. It is demonstrable also, that the ponderable parts of oxygen and hydrogen gas constitute water. There is strong evidence that these gases consist of a peculiar species of matter which is ponderable; and of imponderable matter, which is that which is separable from them in the state of fire, or flame. If fire could be applied in a sufficiently dense state and quantity, it is warrantable, from a full induction of facts, to conclude, that it is able to disunite the constituent substances of all the compound subtances in nature.

The fire of the electric discharge, in a very condensed state, passes with inconceivable velocity through the whole length of the upper wire, in the case of the interrupted explosion, and of the single wire in the case of the complete explosion; so that it neither exerts its energy on the wire, nor on the water, till it arrives at the extremity of the wire. There it is momentarily interrupted and accumulated; and, in the moment before its diffusion through the water, it is so dense and in such quantity as to manifest itself by a spark at, or nearly in contact with, the extremity of the wire. In the moment of its diffusion, a small part of this condensed wire interposes betwixt the constituent elements of the ultimate and invisible particles of water, that is, betwixt the hydrogen and oxygen, of which water is compounded, so as to place them beyond the sphere of their chemical attraction for one another; and each ultimate particle of hydrogen and of oxygen uniting with a determinate quantity of fire, new compound ultimate

particles, consisting of hydrogen and caloric, and of oxygen and caloric, that is, hydrogen gas and oxygen gas, are compounded. This mode of action of electric fire on water is confirmed by the effect of electric fire and common fire, of a due degree of density, on oxide of quicksilver, which is by them resolved into oxygen gas, and quicksilver in the vapour state. All calculations must needs be extremely vague; yet, perhaps, some elucidation will be obtained by considering that as it probably requires seventy or eighty thousand discharges to produce a cubic inch of gas from the supposed decompounded water, the gas produced by each discharge cannot amount to one 200,000th of a grain weight. The quantity of condensed fire at the extremity of the wire must be immensely great, comparatively with the quantity of it which enters into the composition of the gases from decompounded water; otherwise it is not easy to conceive how even the minutest particle of water could be decompounded, the electric fire being in contact with a large body of water, and passing through it with a velocity which is incalculable.

The reason of the metal wire not uniting to the oxygen of the decompounded water, as in the experiment of passing water through red hot iron tubes, might be assigned from the intensity of the fire only; but it is also on account of the rapid motion of the discharge, as well as partly from the great quantity of light. In a very low temperature, light decompounds oxide of silver, and of several other metals; also oxy-muriatic acid, nitric acid, &c. Hence light both decompounds bodies, and prevents oxygen from coming within the sphere of chemical attraction of the metal.

It is supposed that but a very minute proportion indeed, of the electric discharge, is consumed at the end of the wire in the composition of gases, during a momentary interruption, as above said; for it diffuses itself through the water, between the wires, yielding a volume of vivid light till it arrives at the extremity of the under wire or point of the convexity of the metal cup, where it is again condensed by the superior conducting power of the metal to that of water, and where, if it be in due quantity, and of sufficient density, it manifests itself by a spark, and infallibly again decompounds water: hence bubbles are seen to rise from the end of the lower wire, and from the metal cup, as well as from the point of the upper wire.

From these interpretations it will not be difficult to explain the reason of a spark appearing in some cases at the points of both the upper and under wire; why in other cases it appears at the point of the upper wire only; why in others it appears at the point of the lower wire only; namely, according to the density of the fire of the discharge there accumulated.

Concerning the agency of electrical fire in causing the hydrogen and oxygen gas of the supposed decompounded water to undergo combustion and produce water, it is well known that the smallest visible spark, or particle of flame, or fire, can kindle as rapidly a

very large quantity of hydrogen and oxygen gas, as the greatest quantity of flame, sparks, or fire, can kindle the smallest quantity of these two gases; while, on the other hand, the largest mass of matter, heated most intensely, but short of ignition, cannot produce combustion of oxygen and hydrogen gas.

Although, as hath been above explained, caloric and light, in a sufficiently dense state, may decompound every compound substance in nature; it is also well ascertained, that caloric, in certain states, universally promotes chemical combinations. The mode of agency, in this latter case, I apprehend to be in a different way from that commonly accepted; for I think it is unnecessary to suppose that it operates by increasing the power of chemical attraction, and I conceive its agency to be merely diminishing or destroying the powers which counteract chemical union; especially diminishing cohesive attraction, and exciting motion among the particles of the different substances: hence these substances are applied to one another, within their spheres of chemical attraction; or the chemical attraction acts between a greater number of points of the different bodies, as when caloric renders solids and inelastic fluids into the elastic fluid state.

Accordingly, when an electric spark, or the smallest particle of flame, or of an ignited substance, is applied to the gas produced in the above process, or to the mixture of hydrogen and oxygen gas, the ultimate particles of these gases nearest to the flame are driven from it in all directions, as from a centre, by the interposition of fire, or of caloric and light; so that they are brought within the sphere of their chemical attraction for the ultimate particles of the gases at a certain distance from the centre of application of fire; which therefore unite, and the caloric and light, disengaged by that union, act in a similar manner in producing union among the next set in order of proximity of the ultimate particles of the gases; the disengaged caloric and light of which act in producing union of the next set of ultimate particles in order; and so on successively, but with incalculable velocity, the greatest bulk of ultimate particles of the two gases unite with one another; the known products of which union are fire and water; or light, caloric, and water. According to this hypothesis, if caloric or fire, or merely caloric of sufficient intensity and quantity, be applied to a given bulk of hydrogen and oxygen gas, no combustion should be produced; as the caloric will be interposed in such quantity that all the ultimate particles must be at the same instant driven from one another in all directions, so that they are beyond their spheres of chemical attraction for one another; and in various instances common experience verifies this hypothesis.

If light be considered as a different species of substance from caloric, then the theory of its agency, lately published by Dr. Parr, M.D., may be applied very happily to explain the explosion from the combustion of oxygen with hydrogen gas. From a very large induction of facts, Dr. Parr infers that, although light and caloric

subsists together very commonly in the same compound substance, they simultaneously repel each other on the decomposition of the substances with which they were united; and from this repulsion, and also the repulsion of oxygen and light of one another, he chiefly accounts for the combustion of oxygen gas.

On my principles above stated, we can explain why quicksilver of the temperature of 1000°, or more, of Fahrenheit's scale, cannot unite to the oxygen of oxygen gas; but why at this temperature caloric separates oxygen in the gas state from oxide of quicksilver, and why at the temperature of between 600° and 1000°, quicksilver does unite to the oxygen of oxygen gas, but at which temperature oxygen gas is not separable from the oxide of this metal: and again, why at below 600°, oxygen of oxygen gas can neither unite to quicksilver, nor be separated from its oxide.

These principles may be applied variously to the interpretation of the phenomena of combustion, and other cases of chemical combination, according to the state of aggregation of the substances which have a chemical attraction for one another. For example; the oxygen of oxygen gas cannot unite to the constituent substances of a wax or tallow candle, in a low temperature; because the cohesive attraction of the wax or tallow, as well as the chemical attraction between their constituent substances—hydrogen and carbon—counteract the chemical attraction between the oxygen of oxygen gas, and the ultimate particles of hydrogen and carbon of the candle. But at a pretty elevated temperature, when the wax and tallow are in the vapour state, the cohesive attraction no longer subsisting among the ultimate particles of these substances, and the motion excited by the ignited portion of wax or tallow bringing the ultimate particles of these substances within the sphere of chemical attraction of the particles of oxygen gas, these latter unite first to the carbon, by virtue of the stronger attraction between the oxygen and carbon, than that between the oxygen and hydrogen; but, in the instant of this disengagement of the carbon from the hydrogen, a portion of this hydrogen unites also to oxygen, and thus not only carbonic acid gas but water is produced; hence the blue flame, as appears on other occasions, in which there is combustion of hydro-carbonate gas. The remainder of the hydrogen of the decompounded wax or tallow, ascending in the gas state, it unites to the oxygen of oxygen gas; and, as in other cases of combustion of oxygen gas with hydrogen, a white or straw-coloured flame is produced. The wick answers the purpose, by means of capillary attraction, of applying the wax and tallow in such quantities as can be decompounded and combine with the oxygen of atmospherical air; hence the combustion is gradual and equal. The wick itself contains hydrogen and carbon; hence, in combining with oxygen of oxygen gas, it also produces a blue flame. Hydro-carbonate gas being specifically heavier than hydrogen gas, is another reason for the blue flame appearing distinct from the white, and at the inferior part of the *frustum* of a cone of flame

from a burning candle. It is scarcely necessary to say that hydrogen and carbon are constituent substances of wax and tallow; and that when they combine with oxygen, as above explained, the products are water and hydro-carbonate gas.

If I did not use the term demonstration in a more strict and precise sense than is usual, except in mathematics, I would venture to affirm that this theory is almost demonstrated by the agency of fire and carbon on black oxide of manganese. If colourless vitrifiable matter, and that oxide be melted together, by means of the white part of the flame of a candle, purple coloured glass will be produced; for then the hydrogen of this part of the flame carries off little or none of the oxygen from the oxide: but if these substances be melted together by the blue part of the flame, colourless glass will be produced; for then the carbon of this part of the flame carries off oxygen from the black oxide, and produces white oxide. By combining oxygen anew with colourless glass, containing manganese, it will become purple: and this is effected by melting such colourless glass with a little nitrate of potash, or by melting it in open vessels by the yellow flame of a candle. By separating oxygen from glass rendered purple by manganese, colourless glass is produced: and this is effected by melting such coloured glass with a little carbon, or by the blue flame of a candle. I have not thought it necessary to distinguish between the indigo and violet rays; or to notice the extremely thin film of violet flame, which an attentive observer may perceive surrounding the inferior part of the white, extending as high but scarcely higher than the wick, because the explanation is perfectly obvious from the preceding distinction of the two kinds of gases afforded by the candle. If I have explained more satisfactorily than former members of this society the above phenomena of combustion, I owe this advantage to some experiments of combustion of inflammable gases which I have made for some years past, in my chemical lectures; by which the colours are shown to be very different, and correspond to the above theory. I apprehend the theory of an ingenious member of this society cannot explain adequately the phenomena, and does not appear supported by any facts: for there is no evidence that the white flame is not equally the effect of immediate decomposition as well as the blue; or that the blue becomes white flame by ignition; and I presume that the experiments which I have mentioned show that these differences depend upon different decompounding substances contained in the candle.

With regard to the evidence afforded by the foregoing experiments concerning the composition of water and of hydrogen and oxygen gas: these substances are now accounted for in two ways only; namely, 1. By saying that these two gases consist of water and imponderable matter; and that during combustion the water is precipitated. 2. By saying that the two gases consist of a peculiar basis, one of which is named oxygen and the other is hydrogen, each of which is rendered into the gas state by uniting to caloric,

and perhaps also to light; and that during combustion these bases unite with one another, thus compounding water and discharging caloric and light. If complete demonstration could be given, there would not be two opinions; for its proofs, if understood, command universal assent: but the case being otherwise, that opinion must be adopted on the side of which the evidence preponderates according to the laws of reasoning in physical science. Now with regard to the former of these opinions, I can perceive but two facts in support of it. The first of these is supposed to afford a sort of synthetical proof: it is the instance of water being required to obtain, by fire, the whole of the carbonic acid from carbonate of baryt. Here the fact, if admitted, is true only of carbonic acid; but even in this case it has not been shewn that the water enters into the composition of carbonic acid; and until that is proved, it is warrantable to suppose that the water serves to disengage the carbonic acid, by attracting and uniting with the baryt; especially as this acid gas may be wholly separated from other substances without the intermediation of water, and as it can be compounded by uniting the driest carbon to the driest oxygen gas. Further, although carbonic acid gas be very apt to contain water, it may be obtained in so dry a state as not to indicate moisture to the most delicate tests.

The next fact affords a sort of analytical proof; which is the existence of water in air or gas in general, as shown by muriate of lime, acetite of potash, sulphuric acid, quicklime, potash, &c. But the absorption of water by these substances only shews that it may be suspended or dissolved in gases; but not that it enters into their composition. Gas may be obtained or rendered quite free from water, as just said; and as the compounds by the combination of the gases with various different substances are quite different from the compounds by the combination of water with the same various different substances, there does not appear to be in this case any admissible evidence of water being an essential constituent substance in the composition of gases.

The experiments of separating oxygen and nitrogen gas from water by boiling, and by exposing it in vacuo and applying caloric, only shew how much more tenaciously, and in how much greater quantity, these gases are contained in water than is commonly supposed; for however long a given quantity of water yielded gas, so that there seemed no limit to the quantity, the water which afforded it was still very considerably greater than the weight or quantity of gas.

With respect to the other opinion, that hydrogen and oxygen gas are compounded of a peculiar basis and caloric, and that water is compounded of these two bases—1. It has been shewn by an experiment, the accuracy of which is not questionable, that when these two gases produce water by combustion, the amount of the weight of the water is exactly the weight of the gases consumed, and that no ponderable matter but water is produced when nothing but the

bases of these two gases unite; but that when other substances, such as nitrous acid and carbonic acid, were produced, as well as water, then nitrogen and carbon were present. 2. It has been shewn, that when oxygen in a concrete state, as in metallic oxides, unites to the hydrogen of hydrogen gas, the water thus compounded is equal in weight to that of the hydrogen gas consumed, and the deficient weight of the oxide. 3. It appears that when hydrogen in a concrete state, as in alcohol, is united to the oxygen of oxygen gas, the water compounded is equal in weight to the weight of the alcohol and oxygen gas consumed, provided the addition be made of the carbonic acid also produced by the carbon of the alcohol. The carbon of alcohol can be shewn by analysis; and its existence in carbonic acid can be shewn both by analysis and synthesis. 4. When water is applied to certain substances, nothing but hydrogen gas is obtained, and new compounds, the constituent parts of which are oxygen and the substance applied; for these new compounds have the same properties as the compounds of the same substances so applied to water, and of the oxygen of oxygen gas. And in these cases the additional weight derived from the water, together with the weight of the hydrogen gas separated from the water, is equal to the water destroyed. 5. It has been rendered at least very probable, that when oxygen and hydrogen are rendered into the gas state, they absorb or unite with a large quantity of caloric, or of both caloric and light. I might add, that besides the positive evidence just stated, there does not appear to be any evidence to contravene the conclusion that water is compounded of hydrogen and oxygen; and that these gases are compounded of a peculiar basis, called hydrogen and oxygen, united to caloric: for it would be easy to show, but it would be digressing too far, that the remaining partizans of the phlogistic sect have advanced erroneous evidence in some cases, and in others have neglected the consideration of opposing but well authenticated evidence.

In the experiments contained in this paper no support can be found for the former opinion concerning water, and hydrogen and oxygen gas; but they confirm the latter opinion. For, 1. the combustion of these gases, rendered perfectly dry, afforded water: Exp. IV. 2. The evidence from the process above described is peculiar; for no one other process affords oxygen and hydrogen gas from water. In all other processes for decompounding water, either the decompounding concrete substance receives something from the water applied, and at the same time hydrogen gas is produced; as in the instance of passing water through a red hot iron tube: or the decompounding concrete substance receives something from the water; and this compound unites to that in water, which produces hydrogen gas with caloric; as in the instance of applying water to red hot carbon; or two decompounding substances being applied, each of them receives a different thing from the water, namely, hydrogen and oxygen. Hence, by the process above

described, the objection is removed, that the hydrogen gas might be separated from the decompounding substance itself, by water taking its place. 3. The production of hydrogen and oxygen gas, and the production of water by the combustion of them, in the above process, afford an additional evidence of the decomposition and composition of water, and of hydrogen and oxygen gas; as the mode of their production by this process is perfectly consistent with the rationale of their production in all other cases.

Thus it appears that the grandest discovery ever made in chemical philosophy, by an illustrious member of this Society, in 1781, has been confirmed by a number of subsequent experiments. The body of evidence is indeed so numerous, and of such a nature, that, in the minds of those who understand its import, and who rely on the accuracy of the weights and measures employed, it produces as much conviction concerning the composition of water as can be obtained by the evidence of almost any other case of composition. I must, however, beg leave to protest against those able philosophers, who have maintained, that the composition of water, and several gases, has received full and complete demonstration; by such unwarrantable pretensions their adversaries have obtained over them some advantages. For in the chain of causes and effects there are some links which cannot be explained by the direct evidence of sense; and there, in so far as we admit hypotheses, although consistent with the phenomena, we may be said to quit day-light, notwithstanding we at the next step emerge into light derived from the perceptions of sense. For instance, if I combine a certain known weight of hydrogen and oxygen by combustion of their gases, and produce water equal in weight to that of these gases; and if I again resolve this water, by means of the electric fire, or red hot iron, into the original quantity of hydrogen gas and oxygen, I cannot give the full and complete demonstration of the composition water and these gases; for, as I proceed in the interpretation, I at length come to demonstrate the mode of agency of the particles of the hydrogen and oxygen gas on one another when they produce water, caloric, and light. But here I must call in the aid of the imagination; accordingly I imagine that the gases consist of hydrogen and oxygen, which are ponderable, united to caloric, and perhaps light, which are imponderable; that these ponderable particles unite with one another, and their caloric and light are set at liberty. Now here I have not any evidence of sense: for I cannot perceive, by the senses, the existence of the composition of the gases just stated, nor of their decomposition, and union of their ponderable parts. This being the case, other kinds of imponderable may unite, or escape, besides caloric and light; consequently I cannot give the full and complete demonstration in these instances. But the same objections may be made to the pretensions to demonstrate fully and completely the composition of sulphate of soda, sulphate of pot-ash, or any double salt, perhaps, whatever: for caloric is separated, and possibly

other imponderable matter, when these substances are compounded; and caloric and perhaps other matter, may unite when the sulphuric acid and alkali are disunited.

Hence chemistry, in its present state, ought not to pretend to vie with mathematical philosophy in its demonstrations. But it does not appear improbable, that the same certainty as in mathematics, may hereafter be attained in chemistry. We are encouraged to entertain this hope, from finding that the art of observation, and the invention of artifices for rendering the properties of matter evident to the senses, have been proportionate to the advances of knowledge of facts; as was predicted by Chancellor Bacon.

Experiments on the Chemical Production and Agency of Electricity. *By* William Hyde Wollaston, M.D., F.R.S.*

Notwithstanding the power of Mr. Volta's electric pile is now known to be proportional to the disposition of one of the metals to be oxidated by the fluid interposed, a doubt has been entertained by many persons, whether this power arises from the chemical action of the fluid on the metal, or, on the contrary, whether the oxidation itself may not be occasioned by electricity set in motion by the contact of metals that have different conducting powers.

That the oxidation of the metal is the primary cause of the electric phenomena observed, is, I think, to be inferred from the following experiments, which exhibit the galvanic process reduced to its most simple state.

Experiment 1. If a piece of zinc and a piece of silver have each one extremity immersed in the same vessel, containing sulphuric or muriatic acid diluted with a large quantity of water, the zinc is dissolved, and yields hydrogen gas by decomposition of the water: the silver not being acted upon has no power of decomposing water; but, whenever the zinc and silver are made to touch, or any metallic communication is made between them, hydrogen gas is also formed at the surface of the silver.

Any other metal beside zinc, which by assistance of the acid employed is capable of decomposing water, will succeed equally, if the other wire consists of a metal on which the acid has no effect.

Exp. 2. If zinc, iron, or copper, are employed with gold in dilute nitric acid, nitrous gas is formed; in the same manner, and under the same circumstances, as the hydrogen gas in the former experiment.

Exp. 3. Experiments analogous to the former, and equally simple, may also be made with many metallic solutions. If, for instance, the solution contains copper, it will be precipitated by a piece of iron and appear on its surface. Upon silver merely immersed in the same solution, no such effect is produced; but as soon as the

* "Philosophical Transactions" for 1801.

two metals are brought into contact, the silver receives a coating of copper.

In the explanation of these experiments, it is necessary to advert to a point established by means of the electric pile.

We know that when water is placed in a circuit of conductors of electricity, between the two extremities of a pile, if the power is sufficient to oxidate one of the wires of communication, the wire connected with the opposite extremity affords hydrogen gas.

Since the extrication of hydrogen, in this instance, is seen to depend on electricity, it is probable, that in other instances, electricity may be also requisite for its conversion into gas. It would appear, therefore, that in the solution of a metal, electricity is evolved during the action of the acid upon it; and that the formation of hydrogen gas, even in that case depends on a transition of electricity between the fluid and the metal.

We see, moreover, in the first experiment, that the zinc, without contact of any other metal, has the power of decomposing water; and we can have no reason to suppose that the contact of the silver produces any new power, but that it serves merely as a conductor of electricity, and thereby occasions the formation of hydrogen gas.

In the third experiment also, the iron by itself has the power of precipitating copper, by means, I presume, of electricity evolved during its solution; and here likewise the silver, by conducting that electricity, acquires the power of precipitating the copper in its metallic state.

The explanation here given receives additional confirmation from comparative experiments which I have made with common electricity; for it will be seen, that the same transfer of chemical power, and the same apparent reversion of the usual order of chemical affinities in the precipitation of copper by silver, may be effected by a common electrical machine.

The machine with which the following experiments were conducted, consists of a cylinder seven inches in diameter, with a conductor on each side, sixteen inches long, and three and a half inches diameter, each furnished with a sliding electrometer, to regulate the strength of the spark received from them.

Exp. 4. Having a wire of fine silver $\frac{1}{120}$ of an inch in diameter, I coated the middle of it for two or three inches with sealing-wax, and by cutting through in the middle of the wax, exposed a section of the wire. The two coated extremities of the wire thus divided, were immersed in a solution of sulphate of copper, placed in an electric circuit between the two conductors; and sparks, taken at $\frac{1}{10}$ of an inch distance, were passed by means of them through the solution. After 100 turns of the machine, the wire which communicated with (what is called) the negative conductor, had a precipitate formed on its surface, which, upon being burnished, was evidently copper; but the opposite wire had no such coating.

Upon reversing the direction of the current of electricity, the order of the phenomena was of course reversed; the copper being

shortly re-dissolved by the assistance of the oxidating power of positive electricity, and a similar precipitate formed on the opposite wire.

Exp. 5. A similar experiment made with gold wires $\frac{1}{100}$ of an inch in diameter, in a solution of corrosive sublimate, had the same success.

The chemical agency, therefore, of common electricity, is thus proved to be the same with the power excited by chemical means; but, since a difference has been observed in the comparative facility with which the pile of Volta decomposes water, and produces other effects of oxidation and de-oxidation of the bodies exposed to its action, I have been at some pains to remove this difficulty, and can at least produce a very close imitation of the galvanic phenomena by common electricity.

It has been thought necessary to employ powerful machines, and large Leyden jars, for the decomposition of water; but when I considered that the decomposition must depend on duly proportioning the strength of the charge of electricity to the quantity of water, and that the quantity exposed to its action at the surface of communication depends on the extent of that surface, I hoped that, by reducing the surface of communication, the decomposition of water might be effected by smaller machines, and with less powerful excitation than have hitherto been used for that purpose; and in this hope I have not been disappointed.

Exp. 6. Having procured a small wire of fine gold, and given it as fine a point as I could, I inserted it into a capillary glass tube; and after heating the tube, so as to make it adhere to the point and cover it in every part, I gradually ground it down, till with a pocket lens, I could discern that the point of the gold was exposed.

The success of this method exceeding my expectations I coated several wires in the same manner, and found that when sparks from the conductors before mentioned were made to pass through water by means of a point so guarded, a spark passing to the distance of one-eighth of an inch would decompose water, when the point exposed did not exceed $\frac{1}{700}$ of an inch in diameter. With another point, which I estimated at $\frac{1}{1500}$, a succession of sparks $\frac{1}{20}$ of an inch in length, afforded a current of small bubbles of air.

I have since found that the same apparatus will decompose water, with a wire $\frac{1}{40}$ of an inch diameter, coated in the manner before described, if the spark from the prime conductor passes to the distance of $\frac{4}{10}$ of an inch of air.

Exp. 7. In order to try how far the strength of the electric spark might be reduced by proportional diminution of the extremity of the wire, I passed a solution of gold in *aqua regia* through a capillary tube, and, by heating the tube expelled the acid. There remained a thin film of gold, lining the inner surface of the tube, which, by melting the tube, was converted into a very fine thread of gold, through the substance of the glass.

When the extremity of this thread was made the medium of

communication through water, I found that the mere current of electricity would occasion a stream of very small bubbles to rise from the extremity of the gold, although the wire by which it communicated with the positive or negative conductor, was placed in absolute contact with them. Hence it appears, that decomposition of water may take place by common electricity as well as by the electric pile, although no discernible sparks are produced.

The appearance of two currents of air may also be imitated, by occasioning the electricity to pass by fine points of communication on both sides of the water; but, in fact, the resemblance is not complete; for, in every way in which I have tried it, I observed that each wire gave both oxygen and hydrogen gas, instead of their being formed separately, as by the electric pile.

I am inclined to attribute the difference in this respect to the the greater intensity with which it is necessary to employ common electricity; for, that positive and negative electricity, so excited, have each the same chemical power as they are observed to have in the electric pile, may be ascertained by other means.

In the precipitation of copper by silver, an instance of de-oxidation (or phlogistication) by negative electricity has been mentioned; the oxidating power of positive electricity may be also proved, by its effect on vegetable blue colours.

Exp. 8. Having coloured a card with a strong infusion of litmus, I passed a current of electric sparks along it, by means of two fine gold points, touching it at the distance of an inch from each other. The effect, as in other cases, depending on the smallness of the quantity of water, was most discernible when the card was nearly dry. In this state a very few turns of the machine were sufficient to occasion a redness at the positive wire, very manifest to the naked eye. The negative wire, being afterwards placed on the same spot, soon restored it to its original blue colour.

By Mr. Volta's apparatus the same effects are produced in a much less time.

Besides the similarity which has thus been traced between the effects of electricity excited by the common machine, and those observed from the electric pile, I think it appears also probable, that they originate from the same source.

With regard to the latter, its power is now known to depend on oxidation; so also does the excitement in the former appear very much to depend on the same process; for—

Exp. 9. I have found that, by using an amalgam of silver or of platina, which are not liable to be oxidated, I could obtain no electricity. An amalgam of tin on the contrary affords a good degree of excitement. Zinc acts still better; but the best amalgam is made with both tin and zinc, a mixture which is more easily oxidated than either metal separately.

Exp. 10. But, as a farther trial whether oxidation assists in the production of electricity, I mounted a small cylinder, with its cushion and conductor, in a vessel so contrived, that I could at pleasure change the contained air.

After trying the degree of excitement in common air, I substituted carbonic gas, and found that the excitement was immediately destroyed, but that it returned upon re-admission of atmospheric air.

In conformity to this hypothesis, we find that the metal oxidated is, in each case, in a similar state of electricity; for the cushion of the machine, by oxidation of the amalgam adhering to it, becomes negative; and in the former manner, zinc, oxidated by the accumulated power of an electric pile, or simply by action of an acid, is also negative.

This similarity in the means by which both electricity and galvanism appear to be excited, in addition to the resemblance that has been traced between their effects, shews that they are both essentially the same, and confirms an opinion that has already been advanced by others, that all the differences discoverable in the effects of the latter, may be owing to its being less intense, but produced in a much larger quantity.

Extract of a Letter from M. HUMBOLDT *to* M. BLUMENBACH, *containing new Experiments on the Irritation caused by the Metals with respect to their different Impressions on the Organs of Animals.**

M. HUMBOLDT is one of the philosophers who has made the most numerous observations on the phenomenon discovered by Galvani, concerning the irritability produced by the contact of different metals with the parts of animals in which the principle of life is apparently extinguished. As long ago as the year 1795, he observed that the animal irritability was augmented by the oxygenated muriatic acid. Not having discontinued his attention to this object, the perusal of the physiological writings of Reil, and his correspondence with Scarpa and Volta, afforded him indications for new enquiries, of which he has occasionally had the courage to make himself the subject.

"In a conversation," says he, "with M. Scarpa, at Pavia, on the effects which galvanism produced upon myself, nothing surprised him more than the appearance of a lymphatic and serous humour on my back. 'What can be the nature,' said he, 'of this stimulant, which in a few instants changes the nature of the vessels to such a degree as to cause them to prepare humours, which, the instant they touch they epidermis, excite inflammation, and mark their course by a redness which lasts for whole hours?'" M. Humboldt promised to repeat the experiment; and the account he gives of the facts constitutes one of the most interesting articles in his letter.

For this purpose he caused two blistering plasters to be applied on the deltoid muscle of both shoulders. When the left blister was opened, a liquor flowed out which left no other appearance on the

* From Gren's "Journal of Natural Philosophy."

skin than a slight varnish, which disappeared by washing. The wound was afterwards left to dry up: this precaution was necessary, in order that the acrid humour which the galvanic irritation would produce might not be attributed to the idiosyncrisis of the vessels. This painful operation was scarcely commenced on the wound, by the application of zinc and silver, before the serous humour was discharged in abundance: its colour became visibly dark in a few seconds, and left on the parts of the skin where it passed, traces of a brown inflamed red. This humour having descended towards the pit of the stomach, and stopped there, caused a redness of more than an inch in surface. The humour, when traced along the epidermis, left stains, which after having been washed, appeared of a blueish red. The inflamed places having been imprudently washed with cold water, increased so much in colour and extent, that M. Humboldt, as well as his physician Dr. Schalleru, who assisted at these experiments, entertained some apprehension for the consequences.

M. Humboldt has not undertaken to determine the nature of the fluid which produces such astonishing effects; but he applies himself to circumscribe the phenomena in the real circumstances which produce them. He judiciously varies the preparations, and carefully notes all the results; being persuaded that the cause of galvanism cannot be explored with success, but by observing the proportions in which the chain of metals either irritates or has no effect: and to extend still more this vast field of observation, he employs various means to raise or diminish the irritable capacity of the animal organs.

What is the sensation which the galvanic irritation produces? M. Humboldt has discussed this question. "No one," says he, "can speak more decidedly on this subject than myself, having made several experiments on my own person, the seat of which, in some instances, was the socket of a tooth which I had caused to be extracted; in others, certain wounds which I made in my hand; and in others, the excoriations produced by four blistering plasters." The following is his answer:—

The galvanic irritation is always painful, and the more so in proportion as the irritated part is more injured, and the time of irritation more prolonged. The first strokes are felt but slightly; the five or six following are much more sensible, and even scarcely to be endured, until the irritated nerve becomes insensible from continued stimulus. The sensation does not at all resemble that which is caused by the electric commotion and the electric bath; it is a peculiar kind of pain, which is neither sharp, pungent, penetrating, nor by intermissions, like that which is caused by the electric fluid. We may distinguish a violent stroke, a regular pressure, accompanied by an unintermitting glow, which is incomparably more active when the wound is covered with a plate of silver, and irritated by a rod of zinc, than when the plate of zinc is placed on the wound, and the silver pincers are used to establish the communication.

When the communication is made by the contact of the epidermis, it produces no effect; it appears to insulate like glass, when interposed between the wound and the metal; but if the skin be removed, by making two wounds at eight inches distance, and a plate of zinc be placed on one of them, and on the other a leg of a frog prepared, this last is seen to contract itself when it communicates with the zinc by the silver wire, which proves that the galvanic fluid there passes beneath the epidermis.

This fluid produced in some circumstances a very sensible acid taste. The two wounds of M. Humboldt having been covered, one with silver, the other with zinc, an iron wire of several feet in length, attached to the zinc, was conveyed between his upper lip and the spongy substance of the teeth, and thence to the tongue of another person. When the iron wire was made to touch the silver, a strong contraction of the scapular muscle took place, and at the same instant the person whose tongue formed part of the chain of communication perceived the sensation of acidity. There are also cases in which the fluid acts on the organs of taste without producing any sensible effect on the organs of motion: such is that where the epidermis serves as the conductor from zinc to the frog; for there is not then any contraction, but merely an acid taste on the tongue.

The author, having learned from M. Volta that he employed the solution of potash *(oleum tartari per deliquium)*, in order to augment the conducting power, availed himself successfully of this means to raise the capacity of the animal organs. He moistened one of his wounds with this liquor, which produced little pain; but the galvanic irritation was more violent, and accompanied with more heat; sparks appeared and disappeared before his eyes; the tongue moistened with the same, distinctly perceived the acid sensation, although the communication was established only between zinc and zinc. The thigh of the frog, moistened with the alkaline solution and laid upon a plate of glass, without touching either metal or carbonic matter, fell of itself into violent convulsions, the antagonist muscles of the legs and toes being incessantly agitated. Irritability has been re-established by this application in the animal parts, where it had been extinguished by warm solutions of the oxide of arsenic. Lastly, the irritation (which does not commonly take place when the nerve and the muscle are armed with the same metal, the different metals being between the coatings), becomes manifest after this preparation; which seems to indicate that the alkali not only irritates the nerve, but likewise adds to its irritability.

The author applied this method to amphibious animals, which he roused from their winter sleep, and in which he perceived a peculiar symptom of irritability.

These observations led him to distinguish two states of the animal organ. The first, of irritability naturally or artificially raised or excited; the second, irritability in a less degree. These two states,

which he calls *positive* and *negative*, are merely, as he remarks, different degrees, and not phenomena absolutely distinct from each other.

In individuals naturally sensible, the effects produced by alkaline solutions, by the oxygenated muriatic acid, by the solution of oxide of arsenic, are very rarely of the same intensity.

In the case of increased irritability, muscular motions are observed without metal or carbonic matter. They may be obtained with metals, though without communication between the nerve and the muscle; that is to say, without the regular connection or chain. They may be also obtained by forming the chain of similar metals.

Let the crural nerve of an animal naturally tenacious of life be placed upon glass. Let a small piece of fresh muscular flesh be fixed on a stick of sealing-wax, and then brought into contact with the crural muscle. The result will be a violent convulsion at the instant when the chain of communication is completed. The same thing happens if, instead of the small piece of muscular flesh, a detached piece of the crural nerve be fixed on the stick of sealing-wax. The connection is therefore formed of two things, nerve and muscular fibre. How in this simple case can the fluid which passes from the nerve into the muscle cause it to be contracted? M. Humboldt thinks that it becomes stimulant, merely because it returns from the nerve into the nerve by a foreign animal matter; that is to say, not organically connected with the nerve.

The disparity of the metals forming the chain has hitherto appeared as a necessary condition to produce galvanic irritation. This hypothesis, however, is overturned by the experiments of M. Humboldt. If it be true that, in the state of less irritability, there is very rarely contraction with similar metals (as Volta affirms, contrary to Aldini), this circumstance becomes indifferent in the case of increased irritability. M. Humboldt put into a china cup some mercury exactly purified; he placed the whole near a warm stove, in order that the entire mass might assume an equal temperature: the surface was clear, without the appearance of oxidation, humidity, or dust. A thigh of a frog, prepared in such a manner that a crural nerve and a bundle of muscular fibres of the same length hung down separately, was suspended by two silken threads above the mercury. When the nerve alone touched the surface of the metal, no irritation was manifested; but as soon as the muscular bundle and the nerve touched the mercury together, they fell into convulsions so brisk that the skin was extended as in an attack of tetanus.

We ought not to be surprised at the precaution here taken by M. Humboldt to heat the mercury. This is required in consequence of the opinion which he announces, that the parity of the metals does not depend on the homogeneity of their chemical constituent parts, but of their heat, polish, hardness, and force.

Gold, placed between two armatures of zinc, produces irritation only when the gold is moistened by some volatile fluid, or by the moisture of respiration.

Lastly, M. Humboldt has attempted to include all the cases in the following formula:—

1. In the State of Increased Irritability.

Positive cases.
- Frog—muscular flesh.
- Frog—zinc—zinc.
- Frog—zinc—muscular flesh—silver.
- Frog—zinc—silver—zinc.
- Frog—muscular flesh—silver—zinc.
- Frog—zinc—muscular flesh—silver—muscular flesh—zinc.

2. In the State of Diminished Irritability.

Positive cases.
- Frog—zinc—silver.
- Frog—zinc—muscular flesh—silver—zinc.
- Frog—zinc—muscular flesh—silver—muscular flesh—silver—zinc.

Negative cases.
- Frog—zinc—zinc.
- Frog—zinc—muscular flesh—silver.
- Frog—zinc—muscular flesh—silver—zinc.

M. Humboldt finished this letter by some observations which he has collected in the course of his experiments on the *sthenic* or *asthenic* virtue of chemical agents; that is to say, their energy or their inefficacy to produce irritation. Alkalis appear to be to the sensible fibres what acids are to muscular groups. The muriatic acid augments the irritability of the muscle while it extinguishes that of the nerves, which does not re-appear even after the acid has been saturated with alkali.

By continuing to bathe the nerve with an alkaline solution, an entire atony is at length produced by excess of irritation; but if a few drops of muriatic acid be left on the part, the irritability is re-established.

A thigh of a frog, irritated even to total relaxation by a warm solution of oxide of arsenic, has exhibited new convulsions after having been immersed for two minutes in a solution of potash.

The sthenic virtue of the oxygenated muriatic acid is not less remarkable. Thighs of frogs naturally flaccid, and weakened still more by the galvanic process for seven hours, which afforded no sign of motion when silver served as a conductor between zinc and the nerve, exhibited violent contractions when the nerve was moistened with oxygenated muriatic acid. The author refers to this subject the experiment which he published in 1793, in his *Flora Fribergensis*, by which it is ascertained that ordinary muriatic acid retards the germination of plants, but that oxygenated muriatic acid had caused a plant to germinate in seven hours, which required thirty-eight in pure water in order to arrive at the same development. This fact appears to him to indicate some relation between the vegetable and animal organization.

*Account of certain remarkable Changes of Colour and Direction of Clouds during a Thunder Storm.**

Among other circumstances enumerated by Dr. Priestley in the description of the clouds in a thunder storm, in his "History of Electricity," mention is made of a certain luminous appearance, evidently independent of solar reflection. I have always supposed this expression to denote the opaque whiteness of the upper or arched outline of certain thunder clouds, contrasted with others apparently in contact with them, but of a dull leaden hue; and accordingly I was disposed to conclude that the whole was an optical delusion, arising from the position of the spectator, who imagined, though falsely, that the latter clouds were as much exposed to the sun's direct light as the former. But the storm which happened on Sunday morning, the 30th July last, exhibited facts which seem to show that the transition of electricity may cause the clouds to emit a steady permanent light, very different from the sudden flash called lightning.

I was called at five o'clock in the morning. The sky was then covered with clouds, not very dense except to the south, and flying with great rapidity to the W. by S., or W.S.W. It lightened very frequently in the N.W. and S.W. quarters, by doubled and trebled flashes of a bright illumination (for the actual flash was not seen), with very loud thunder, usually at the interval of eleven or twelve seconds after the flash. The lower prominences, or ragged extremities of the clouds, were constantly tinged with red, and I was informed that they had been very much redder before I got up.

At about ten minutes after five, no rain having fallen, but a few heavy drops, a sudden darkness came on, and the dust rose in Newman-street, where I reside, beginning at the south end, about 250 yards distant from my house, and proceeding to the north. It was very dense, and rose to the height of about sixty feet by estimate, which is much higher than the houses in the street. At about a quarter past five, the darkness being then greatest, the houses on the opposite side of the street appeared as if seen through a deep blue glass, particularly the white stone work above the windows; and upon looking upwards, the clouds were seen of a deep leaden blue colour, and moving swiftly in a direction precisely opposite to that before observed, namely, to the E. by N., or E.N.E. Soon afterwards, the lightning and thunder continuing during the whole of these changes, there fell a heavy shower, which beat against the western face of the house, and the darkness gradually went off. At half-past five the clouds were much higher, and moved with a moderate angular motion to the north, while the smoke of a chimney opposite my window was gently driven to the south.

We have yet much to learn concerning the theory of thunder-

* "Nicholson's Journal."

storms. It is well known by experiments with the electrical doubler,* that almost all bodies possess a certain degree of electrization, which is variable from a considerable number of circumstances. It is also known from Franklin's experiments of the can and chain, as well as from numerous other facts, that the intensity of the electric state will be augmented by diminishing the surfaces of bodies. In this way, as well as from other causes, it is inferred that clouds become highly electrified during the progress of their condensation; so that flashes of lightning pass between them and the earth, and between each other. From other observations and deductions, it has been also rendered highly probable that the long range of clouds in a storm of this nature does serve as a conductor, through which flashes of electric fire are conveyed from one part of the earth to another part in a different state. The facts above described seem uncommon, and, if collated with other more usual events, may afford some instruction concerning this class of phenomena. I do not highly esteem the conjectures which present themselves to me on this occasion; but shall communicate them, because they tend to point out future objects of research.

The singularity of this thunder storm appears to have arisen from the mass of aqueous vapour having been much too small to afford a favourable communication between the two opposite states of electricity on the surface of the earth. If the mass of clouds be supposed to have been at first near the eastern portion of the earth, and to have become electrified and repelled, they would, on the common principle of bodies in that state, be repelled, and pass swiftly to the western part of the surface to deposit their electricity, as soon as they came within the striking distance. In this situation, it may be imagined that the supply or communication might continue to be made by the eastern part of the cloud acting in the manner of a point, while the western part emitted flashes; a supposition which is rendered more probable by the consideration that most thunder clouds are ragged or pointed on one side, and round or swelled on the other; and also that the disposition of any conductor to receive electricity without explosion is much greater than to give silently, even when the terminations are alike. I conjecture, therefore, that the lightning came from the east, and passed through the clouds to the west; that the posterior extremities of the clouds were illuminated as points usually are; that the electric motion of these low clouds at first caused an easterly wind; that some change in the general state of electricity, or perhaps the mere exhausted state of the clouds, caused them to pass rapidly back to the original reservoir, as in the most common experiments of electricity; that this return produced first a strong eddy in the lower air, which threw up the dust, and afterwards a contrary stream of wind, by which the rain was beaten against the west front of the house. But why the

* "New Experiments on Electricity." By the Rev. A. Bennet, F.R.S., 8vo, London.

clouds should have been illuminated with a red colour during their western course, and afterwards with blue, does not seem deducible from any facts I know of. It may perhaps be analogous to the colours of the aurora borealis. The contrary lower current, when the clouds were removing to the north, seems to have been a natural consequence of their remoteness. It is probable that the returning current of the air, which must have been driven before the mass of clouds when they were moving very near the earth, took place entirely in the upper part of the air, where, in that case, there was more room.

On an Aurora Borealis seen at Woolwich, December, 22, 1834. *By* William Sturgeon.

A beautiful aurora borealis was seen from this place last night. I was on Woolwich Common when I first saw it, then exactly six o'clock. It consisted of several groups of vertical beams of pale yellowish light on both sides of the north star, extending nearly to equal distances on the western and eastern directions. These beams presented the strongest light at their bases, and grew gradually fainter, to their superior extremities, where they softened and gently glided into the most attenuated light, and were lost at various altitudes, some of which were near to the zenith. These streamers soon faded, and gave place to a few straggling vertical corruscations, displayed in various parts of the northern sky, which in their turn were again succeeded by the finest streamers I ever beheld. It was now five minutes past six. These splendid streamers were of the same tint as the former, and extended from the black nucleus near the horizon to the zenith in nearly the same manner; but the refulgence of these far exceeded that of the former. These streamers consisted principally of two parallel groups, one on each side of the north, and with some considerable distance between them. Smaller streamers were, however, playing in the intermediate space and also on their outer horizontal skirts. The horizontal boundaries of the aurora, at this time, seemed to be the Milky Way on the west, and near to the planet Mars on the east. From this time the aurora gradually diminished in splendour, and about seven was nearly lost; it occasionally, however, brightened with a few faint flashing momentary streamers till between ten and eleven, at which time I discontinued my observations.

During the display of the fine streamers, which first presented themselves about five minutes past six, I hurried home to adjust a magnetic needle. It was about half-past six before I had my magnetic apparatus fit for observation, and the splendour of the aurora had now passed its meridian. I diligently watched the needle and the aurora till half-past ten, but observed nothing in the motions of the former that could possibly be attributed to the influence of the latter.

From the brilliancy of the aurora at six o'clock, I imagine that it was exhibited at a much earlier period of the evening, but I have had no opportunity of ascertaining the fact from persons likely to have seen it. I think it is likely that the aurora was very fine in Scotland, and perhaps in higher north latitudes, after seven o'clock, perhaps till nine or ten.

W. Sturgeon.

Artillery Place, Woolwich, Dec. 23, 1834.

P.S. This aurora appeared to have no particular respect for the magnetic north: it was nearly, if not exactly bisected by the *true meridian* during the whole of the time I observed it.

Description of the Aurora Borealis of November 16, 1835, *By* William Sturgeon.

An aurora borealis of a very unusual character was seen in this neighbourhood, and I imagine over a large tract of country, on Wednesday evening the 16th instant. I was walking from Greenwich to Woolwich, between nine and ten o'clock, and when I had arrived at the top of Maize Hill, by the side of Greenwich Park, then about ten minutes past nine, my attention was first attracted by the fine light of the aurora in the north. I walked on a little further till a good opening to the northern horzion presented itself from the road leading from Maize Hill to Mr. Angerstein's estate. At this opening I made a determined stand, for the purpose of observing any novel phenomenon which the aurora might happen to present.

At this time it consisted principally of a very extensive lateral range, on both sides of the pole star, of vertical streamers, which were pencilling the northern heavens from about 15° above the horizon to Cassiopeia's Chair, then about the meridian; and so uniform was their arrangement and splendour, that they presented one sheet of yellowish white light, the most intense at the base, and becoming more and more faint as they proceeded upwards, until quite lost at their terminal altitudes.

This appearance of the aurora had but just stamped its impression on my mind, when in one moment the whole of the northern heavens appeared in one complete state of undulating commotion, heaving upwards in rapid succession immense waves of light,* which, like the streamers which preceded them, gradually diminished in

* These waves were seen at Milton, next Gravesend, by my scientific friend Mr. Swinny; and I beg to acknowledge the obligation I am placed under to Mrs. Swinny, who also saw these waves, for a more happy description of them than any I had before thought of. They appeared to this lady as "waves of thin smoke or steam, behind which was placed a strong light." A more expressive description could not possibly be given.

brilliancy from their source near to the horizon till their arrival at the zenith, which was their general vanishing point.

The horizontal range of the aurora during this unusual display, was eastward as far as Jupiter, whose azimuth from the north was then about 75°; and perhaps about the same extent westward on the other side of the pole star. I observed it stretch to beyond α Lyræ (Vega), whose azimuth from the north was about 60°, but could not very well ascertain the position of the western extremity at the place where I was standing, on account of the reflexion of the gas light in London mixing with that of the aurora, and the intervention of trees, &c.

This extensive ocean of light, which illuminated nearly half of the visible heavens, and whose waves rolled with the rapidity of thought, lasted about eight or ten minutes, perhaps longer, when they gradually began to disappear, and the aurora to contract in all its dimensions. Until this time (nearly half-past nine) no dense nucleus had marked the centre of the aurora: the stars were seen between the horizon and the luminous base as decidedly, though not so clear, as if no aurora were present. The star Benetnasch, in the the tip of the tail of the Great Bear (η Ursæ Majoris), was one of those which were observed below the aurora; but Mizar (ζ Ursæ Majoris), then on the meridian below the pole, was seen in the bright arched base of the streamers and waves.

The last-mentioned change in the appearance of the aurora brought it gradually to that state which is usually exhibited in some period or another of this boreal phænomenon. The dense black nucleus began to form, and soon curtained the stars which had previously twinkled in that segment of the northern sky. The luminous margin also, its usual attendant, became well defined, and its highest point was well marked to the westward of the meridian, perhaps nearly in the magnetic north. I now walked on, keeping the aurora in view, which shot occasional streamers from various parts of the luminous arch. Just before I entered Woolwich, about ten o'clock, another fine display of vertical streamers spread over the northern sky, and continned for nearly a quarter of an hour. By this time I reached home; but too late to ascertain their effect, if any, upon the magnetic needle, for they faded away very rapidly after my arrival, and before eleven the aurora had entirely disappeared.

During my walk home, I observed several fine meteoric stars, most of which appeared to be shot from the same point of the heavens, which point was somewhere in a right line between me and the Twins. One of these meteors shot with a moderate velocity across the north part of the meridian, at an altitude of about 80°, and appeared to traverse an arch of the heavens of 90° or 100°. It burst into several luminous fragments at the western termination of its range, and became extinct in a moment. I listened for some time, but heard no noise, neither did my servant who was with me, and who listened attentively at my request. I had previously pointed out to him the direction he was to look in, and he saw the meteor from the

first to its last appearance. He also directly afterwards saw another from the same quarter, which traversed the heavens in nearly the same direction as the former. He called out to me, but it was lost without my seeing it. These meteors was seen above five minutes before the last display of streamers mentioned above.

I saw no appearance of the aurora to the south of the zenith, though frequently looked for. The sky was quite clear of clouds, and the black southern expanse, studded with its brilliant stars, afforded a fine contrast to the display of the aurora in the north.

Artillery Place, Woolwich, Nov. 19, 1835.

N.B. Whilst writing the above, a friend has called on me, who saw fine streamers about half-past eight o'clock.

On Electro-pulsations and Electro-momentum. *By* William Sturgeon.

It is very well known to the readers of the "Philosophical Magazine," that I have long considered electric currents, when transmitted through inferior conductors between the poles of a voltaic battery, as the effect of a series of distinct discharges, in such rapid succession as not to be individually distinguished by the senses. Such currents I have called electro-pulsatory. See my theory of magnetic electricity in the "London and Edinburgh Philosophical Magazine," vol. ii, p. 202.

By following up these views of electro-pulsations, I was about two years ago enabled to dispense with all acid or saline liquids, in the employment of galvanic batteries for the purpose of galvanizing, as it called, either to satisfy the curiosity or as a medical process; and my plan, which answers very well, I have found to be productive of a considerable saving in the expense necessarily attendant on the use of voltaic batteries when excited by acid solutions.

It is well known that a Cruikshank battery of about a hundred pairs will, by employing water alone in the cells, charge to a certain degree of intensity almost any extent of coated surface of glass that we please; and that the same degree of charge is given to it by a single contact of the conductors, however short its duration. This being understood, and understanding also that the shock produced by any discharge from a given intensity would be proportional to the quantity of fluid transmitted in a given time, it was easy to foresee that a series of shocks in rapid succession might be produced by some mechanical contrivance, and that the degree of force might be regulated by varying the extent of coated surface.

My first experiments were made with a hundred and fifty pairs of three-inch plates, and about seven feet on each side of coated glass; and my apparatus for producing a rapid succession of shocks was one of Mr. Barlow's stellated electro-magnetic wheels which was soldered to an iron spindle and put into rotatory motion by a

wheel and a band; the points of the wheel touching in succession a copper spring in connexion with the positive surface, and thus producing a discharge at every contact of the wheel and copper spring.

When the two surfaces are connected by wires with two basins of salt water, and the hands immersed one in each basin, the effect experienced is precisely that of a discharge of a voltaic battery. The discharges can be made in such rapid succession as to prevent the sensation of distinct shocks; and if the process were to be concealed it would require some experience to distinguish between the effects on the animal economy from this apparatus and those from a voltaic battery charged with acid and water.

My views being so far verified, the next attempt was to simplify the apparatus and make it more portable; and as it was readily seen that if one hundred pairs would charge glass of considerable thickness, thinner glass might be charged by fewer pairs; this was done: and eventually the glass entirely dismissed, and its place supplied with well-varnished Bristol-board. These boards answer exceedingly well as a reservoir for low intensities; they may be coated to within an inch of the edge all round, and placed upon their edges either on a piece of glass, or on a board properly prepared, and arranged to any required extent like the plates of a voltaic battery, but when considerable intensity is wanted, it is better to use thin glass.

From these facts we learn that metallic surfaces of many acres of extent may possibly be charged to a low intensity in the interior of the earth, by having a thin intervening stratum of inferior conducting matter sufficient to insulate from each other their dissimilar electric surfaces.

It may now be understood that the slightest accident which would suddenly break through the insulation, such as the sinking of a mass of metalline matter from one stratum to the other, would cause a sudden rush of an immense ocean of the electric fluid, which might be productive of subterranean lightnings and tremendous explosions, sufficient to shake an extensive range of country on every side.

Connected with the preceding facts there are others which may be conveniently mentioned in this place, and which would lead us to similar explanations of the cause of subterraneous convulsions. Electric currents of considerable magnitude when suddenly checked, or diverted to a new channel, produce a momentum not very generally understood; but which I will endeavour to explain. A coil of copper wire, excited by magnetic action, will become a channel for an electric current; and whilst the whole circuit is metallic, the velocity of that current would be considerably greater than if any, even a small part, of the circuit were of worse conducting materials: and if the current were suddenly transferred from a channel of the former character to one of the latter, by any contrivance whatever, it would meet a resistance on entering the new channel, which the momentum it had previously acquired would have to overcome; and a sudden disturbance of the electric fluid,

previously at rest, would take place, and a violent rush of the current would as suddenly follow.

It is in this manner that shocks and sparks are produced by magnetic electric machines, where the current, previously in rapid motion, is suddenly transferred to a new channel of inferior conducting character; and all the fluid in the revolving coil rushes through a person properly situated for the new route, and who experiences the electric shock, or else through a thin stratum of air at an interruption in the metallic circuit where the spark is produced.

These, then, are some of the effects of electric currents, or of the momentum of the electric fluid in a state of motion, after the exciting cause is entirely cut off. The shock thus produced may very conveniently be compared to the blow given by Montgolfier's hydraulic ram. Electro-momenta may be produced by any mode of excitation whatever, and the effects will be proportional to the velocity and quantity of the electric fluid first put into motion; and the length of the original channel is also to be taken into account. If then electro-momenta, capable of producing violent shocks and vivid sparks, can be produced by a few hundreds of feet of thin copper wire, what is it that might not be expected from the electro-momenta of nature, arising from currents of many miles in extent, kept in motion either by heat, saline solutions, or by other causes, amongst the metalline strata below the surface of the earth? A sudden disruption in the circuit would insure the blow, and an earthquake might be the result.

Artillery Place, Woolwich, July 4, 1836.

Letter from M. Edmond Becquerel *to the Editors of the "Annales de Chimie et de Physique."* *

In the "Annales de Chimie et de Physique," for December, 1841, I gave a notice on voltaic batteries of continuous action, in which I stated such facts on the subject as have resulted from the experiments of those natural philosophers who have been engaged in these enquiries.

Mr. Daniell supposing that I had not done him justice, has thought proper to reply to several of my statements in the "Philosophical Magazine," for April, 1842.† It was far from my intention to state any thing displeasing to Mr. Daniell, or to misrepresent facts, or to claim for my father a discovery to which he had no right. In this particular Mr. Daniell has unaccountably mistaken my motives for publishing my notice, and had it not been for this circumstance I should not have thought proper to reply to him at this time; for, as regards the principal points in the facts which I mentioned, I have nothing whatever to alter.

* "Annales de Chimie et de Physique."

† See "Annals of Electricity, &c." Vol. viii, p. 456.

In every physical question, three particulars are to be taken into consideration: the idea, the principle, and the application. Now, it is proved by well ascertained facts, that from 1829, and even several years previously, my father had invented and constructed voltaic batteries of constant action; though it be true that they had not the *extent* of action, and the other advantages possessed by those batteries which Mr. Daniell made known in 1836. The apparatus invented by my father immediately obtained the appellation of "Constant Voltaic Batteries;" and as they still answer the purpose he then intended, to contend with him now about either the *idea*, the *principle*, or the *application*, within certain limits, would be perfectly ridiculous.

Respecting the priority of invention, the details which I am about to give, will, I think, leave no farther doubt on the subject. For more than fifteen years the electro-chemical reactions, by means of which my father was enabled to produce crystallized substances, were carried on by the employment of a small piece of apparatus consisting of tubes bent into the shape of an U, closed at the bended part by a partition of moist clay, for the purpose of keeping separate the two liquids which occupied the branches of each tube: in one of which was placed a solution of sulphate, nitrate, or chloride of copper, in which a plate of copper was immersed. In the other branch was placed a solution of sea salt and a plate of zinc, or some other metal. Such is the arrangement of the simple apparatus which is known to the scientific by the name *Pile à Cloison*.

The figure of this apparatus is of but little importance, as it may be varied at pleasure without altering its principle or mode of action. Instead of an U tube, for example, we may employ any kind of vessel, formed into two compartments by a diaphragm of bladder, baked earth, plaster, linen, or other fabric, &c.; but all such variations are necessarily on the principle of the U tube.

After the year 1829, long before Mr. Daniell published any thing on the subject, my father made several communications on this subject to the Academy of Sciences: and the description of an apparatus of this kind, which maintained a constant current for two entire days, is to be seen in the "Compte Rendu des Séances," for 1835.

According to this fact, therefore, Mr. Daniell can have no pretentions to the discovery of the general *principle* on which the operation of the "*constant voltaic battery*" entirely depends; but he may justly claim the good arrangement that he has given to this battery, and, amongst others, the advantage of preserving a saturated solution of sulphate of copper, and of obtaining more energetic effects than those for which my father had any occasion, at the commencement, for the production of crystallized substances analogous to those formed by nature; a discovery for which he received the Copley Medal from the Royal Society of London, and which Mr. Daniell also received some time afterwards, for his form of the constant voltaic battery.

Notwithstanding these facts, so long before the public, and so self-evident as not to be mistaken, Mr. Daniell declares that, in the construction of his battery, he was not guided by the works of his predecessors, and that the principles which he adopted are different from those which my father had so long since employed. He states, for instance, that the rapid diminution and early cessation of the current by ordinary batteries, are due to the deposition of zinc on the negative plates of each pair. On this point we perfectly agree: the annihilating counteraction occasioned by the deposition of zinc upon the copper plates comes under that called *polarization of the electrodes* (an odd enough expression). In my notice I mention that "each negative plate (of copper or platinum) retains on its surface alkaline elements, such as hydrogen, liberated by the decomposition of water, and bases liberated by the decomposition of saline substances dissolved in the water." This phrase does not exclude any of the bases; therefore, the zinc liberated from the salt of zinc must also be deposited on the negative plate; a fact known many years ago, and by Sir Humphry Davy shown to be the cause of the action of Ritter's "*secondary piles.*" This decomposition being effected, the action of the liquid on the zinc necessarily gives birth to a counter current, which gradually destroys the action of the primary one. Therefore, in order to obtain an apparatus of *continuous action*, it was necessary to devise some means to prevent the zinc and the alkalies being deposited on the negative plates.

Mr. Daniell next says, that the passage of the electric current across the diaphragms of bladder is well known to experimentalists, and quotes Dr. Ritchie as having made use of them. To this I reply, that the use of diaphragms in physics is very ancient, since one of the Bernoullis had already separated the different liquids by a membrane, in an experiment intended to produce *endosmose* effects. Porret also adopted the same means for the purpose of showing that in separating, by means of a membrane, a mass of water into two parts, and by immersing in each a platinum plate connected with the poles of a battery, the current would cause the water in the positive cell to traverse the membrane and arrive in the negative cell. I might still give other examples were it necessary. The use of membranes and other diaphragms for transmitting constant currents was brought into use by my father nine or ten years before Mr. Daniell was engaged in such enquiries; as the experiments communicated to the Academy of Sciences, on the 23rd February, 1829,* will amply testify.

As to the publication of Dr. Ritchie in the "Philosophical Transactions," it is dated May, 1829, and, consequently, some months later. I therefore look upon Mr. Daniell's battery, although very convenient, as based upon the same principles as the apparatus employed by my father for several years previously.

* "Annales de Chimie et de Physique," t. xli.

Farther on, Mr. Daniell adds: "Even in the use of the diaphragm, which might at first sight appear similar, there is a *direct* opposition; for my object is to keep the two electrolytes which I employ, perfectly separate, so that no portion of one may penetrate to the other, except in the process of electrolysis."

I confess that I know not how Mr. Daniell can keep separate two liquids by a membrane moistened by them and which they can penetrate, without that passage from one to the other taking place which is otherwise called *endosmose* and *exosmose*. Indeed, it is impossible to realise this condition. The only means of retarding the mixture of the two liquids, as long as possible, is by substituting for the membrane a thick diaphragm of clay, as did my father: the intensity of the current is certainly diminished, but uniform effects are obtained which may continue for months or even years.

Mr. Daniell further says: "and I repeat, that in my constant battery nothing depends upon the contact and action of the two liquids upon each other."

I do not understand this assertion: for every one knows that two different liquids acting upon each other by an intermediate membrane, disengage electricity enough to produce a current; and if Mr. Daniell wishes to convince himself of the fact, he has only to take away, in one of his pairs, the plates of zinc and of copper, and substitute for them two plates of platinum; he will have a current owing to the reaction of the two liquids upon each other, less intense, indeed, than that derived from a pair, one of which is an oxidable metal.

Mr. Daniell also says that, "the amount of force obtained by my father's apparatus is insignificant as respects its application to the arts."

I will reply *yes* and *no* to him: *yes*, if there be a question of apparatus like that of Mr. Daniell, intended to produce currents which are to be transmitted into liquids placed in separate vessels: *no*, if the currents are to re-act chemically on the liquids which themselves form a part of the apparatus.

In short, the apparatus constructed by my father, six years ago, for the treatment of the ores of silver, lead, and copper, are based on the same principles that I have before explained; and these apparatus are of much more considerable dimensions than that of Mr. Daniell, since each pair requires 1,000 litres of liquid for its excitement, and six of these pairs have been united into one battery: therefore 6,000 litres of liquid have been employed at one time: and the energy of this apparatus has been still greater than that produced by Mr. Daniell's battery: for all the silver and lead contained in these ores, amounting to about one kilogramme of silver, and 100 kilogrammes of lead were extracted in the space of a few hours.

I now leave it to the judgment of the reader who is in the right,

Mr. Daniell or myself: it will then be understood whether *filial piety blinded me*, or that I was actuated by the love of truth.

Paris, July 7, 1842.

[Professor Grove has also sifted Professor Daniell's claims, in the following letter to the editors of the "Philosophical Magazine:"*—]

Allow me to request your insertion of a few remarks on a letter of Professor Daniell, published in your magazine for April. Absence from London and occupations of other than a scientific nature prevented my noticing it at the time; my attention has been recalled to the matter by its republication in the "Annales de Chimie."

A few words at the conclusion of this letter refer to me: after stating that M. Becquerel has inadvertently described my experiments as anterior to Mr. Daniell's, this gentleman goes on to say, "Professor Grove has never spoken of his battery but as the further application of principles which I had previously deduced."

It is perhaps of little moment to the public what principles led me to the construction of the battery in question, but it may be of some moment to me, as should I, by silence, be held to assent to certain principles, I may be accused of contradiction and inconsistency, if in any future paper I should state my adherence to others. M. Becquerel, again, in the 5th volume of his "Traité de l' Electricité," describes my battery as "pile voltaïque construite d'après les principes exposés dans les chapitres 1er, &c.:" these chapters contain the papers of M. Becquerel in respect of which he claims priority to Mr. Daniell. It is obvious, that as M. Becquerel and Mr. Daniell differ in their notions as to the principles of the constant battery, I could not derive my battery from both, and I have looked over my papers on this subject to see whether I have expressly referred it to principles enounced by either of these philosophers: I cannot see that I have. I have on many occasions mentioned their experiments before my own in the history of the voltaic pile, both as acknowledging their priority and as not wishing to claim what was not my due; probably it is this which has led to a misconception on the part of Mr. Daniell; but I have distinctly stated the idea which immediately led to the construction of my battery in the paper which describes it in the "Philosophical Magazine" for May, 1839. After detailing an experiment with two strips of gold-leaf in nitric and hydrochloric acids, separated by a porous diaphragm, and showing that upon contact of the two strips, the gold in the hydrochloric acid was dissolved, and that a voltaic current was established, I say, "It now occurred to me, that as gold, platina, and two acids gave so powerful an electric current," *à fortiori*, "the same arrangement, with the substitution of zinc for gold, must form a combination more energetic than any yet known:" this was the simple deduction which led to my subsequent experiments. I have in most cases been con-

* "Philosophical Magazine."

tent to publish experiments with no more of theory than was requisite to connect them; it is a general, and I think a just complaint, that there are already too many speculations on this subject; but in a letter published in the "Philosophical Magazine" for Feb., 1839, p. 129, previous to the discovery of my battery, I gave my own notions of the principles of voltaic batteries, notions which in some respects agree with those of Mr. Daniell, but which also suggest some new views of voltaic action. There is one experiment there detailed in which copper is reduced by copper, which had much influence on my subsequent experiments, but which is not explicable by any principles laid down by Mr. Daniell. At the conclusion of this paper I say, "if these principles be correct, very superior combinations may be discovered:" how this prediction has been fulfilled the public is the best judge.

Far be it from me to disclaim any assistance from the experiments of Mr. Daniell or of M. Becquerel; I shall ever retain a grateful recollection of the assistance rendered to my first efforts in science by the latter gentleman. I cannot at this distance of time well describe what effect their experiments had upon my mind. In the progress of science it is difficult to define the frequently unperceived effect of prior discoveries upon subsequent experimentalists, but I cannot for many reasons acquiesce in the assertion of Mr. Daniell above quoted.

Mr. Daniell was for a long time attached to the theory of the deposition of metals in the voltaic circuit being the result of a secondary action of the nascent hydrogen, a theory generally adopted until combated by Hisinger and Berzelius; thus in his papers, "Philosophical Transactions," 1836, p. 117, *et seq.*, he proceeds to explain his constant battery as dependent upon the removal of that hydrogen by causing it to deoxidate copper: in a subsequent publication in the "Philosophical Transactions," 1839, he abandons this view, and considers the deposition of the copper as "a primary result of electrolytic action." This would altogether alter the theory of his battery and of mine. I do not think it is a matter of great consequence which theory be adopted; each has many peculiar difficulties, each tends to many similar conclusions, and either may lead to equally successful experimental results. Theory is valuable as a means, not as an end; and that theory of the voltaic battery is, in my opinion, the best which best collates the observed phenomena, and which leads to the discovery of the best voltaic combinations. But although I would hesitate, without more conclusive experiments, in ascribing this superiority to either of these theories, there is another principle of the voltaic battery enounced by Mr. Daniell, as to which, so far from agreeing with him, I must take leave (with every respect for his scientific attainments) to differ *toto cœlo*: it is as to the relative extent of surface to be given to the metals of voltaic combinations. Mr. Daniell has in the "Philosophical Transactions," for 1836, p. 128, and in several subsequent papers, stated that the best theoretical form for a vol-

taic combination is when the generating metal is arranged with regard to the conducting one as the centre of a sphere to its periphery, and recommends a rod within a cylinder as the nearest practical approximation to such an arrangement; following the authority of Mr. Daniell, I first constructed my batteries of this form, but very soon abandoned it: see "Philosophical Magazine," for Oct., 1839, p. 288: and I am now convinced, by three years' experience, and by repeated experiments, corroborated by the experiments of others, that this is by no means the best form of arrangement, as regards economy either of space, time, or material. I believe the old arrangement of equal surfaces to be sufficient for most practical purposes; but the relative size may be considerably modified according to the nature of the electrolytes, the conducting power of the metals, and other circumstances. I cannot enter more fully on this point without writing a paper especially on this subject.

P.S.—Since the above was written I have received a paper of Mr. Daniell's, just printed in the "Philosophical Transactions," 1842, part ii, for which I have to thank the author: it contains a series of experiments on my battery, and with a voltameter of my contrivance. In this paper I see Mr. Daniell alters many of his opinions upon the relative size of the plates in voltaic combinations.

MORE FACTS.

Being now engaged in clearing up accounts, we will take the liberty of bringing before our readers a few more facts, which not only bear on some parts of the above reckoning, but are items in another perfectly distinct account.

One of the essential conditions in the performance of continuous uniform action in a voltaic battery, one of whose liquids is a solution of a salt of copper, is the reduction to the lowest possible point of the *activity of the zinc* on that solution, and also on the solution on which it is immersed. With respect to the solution of the sulphate of copper, the activity of the zinc is considerably abated by the intervention of the diaphragm, but never entirely subdued by the intervention of a thin membrane or paper. Amalgamation of the zinc lessens its electro-chemical energies to a considerable extent, and it is in this state that it becomes so efficient in continuing the action of voltaic batteries. This being understood, we have next to place this grand essential of "the constant battery" to the credit of those who first discovered the great diminution of electro-chemical action of zinc when amalgamated.

In the first volume of these "Annals," we have given an historical sketch of the employment of amalgamated zinc in voltaic batteries, and we have only to refer to that article to satisfy the reader on that point: and with respect to the almost entire extinction of electro-chemical action of amalgamated zinc on a solution of sulphuric acid, the solution used by Mr. Daniell in his battery, a reference to page 88,

of the same volume, might be sufficient. But as Professor Grove has made a claim to the discovery of the inactivity of amalgamated zinc on diluted sulphuric acid, in a paper read in the Academy of Sciences, Paris, it may not be amiss to give, at whole length, the articles from which those extracts were taken; especially as they contain some other theoretical points in the action of voltaic batteries, which Professor Grove has also subsequently discovered. In a paper which appears in Vol. V, of these "Annals," Professor Grove states, in page 254, amongst other things, "But the most remarkable fact is, that the plate of zinc which was in the acid, although much more attacked chemically than that in the alkali, always took positive electricity; *i.e.*, it represented the copper in an ordinary voltaic combination."

We are very far from imputing to Professor Grove any improper motives for not alluding to similar discoveries, because it is probable that he might not have been aware of them at the time that he wrote, although published in the "Annals of Electricity, &c.," nearly four years previously, and originally in a pamphlet, well known in London, ten years previously to the date of Professor Grove's experiments. The pamphlet containing the following facts is dated 1830:—

45. Let two equal slips of sheet zinc, of any convenient size, be polished with glass paper. Let the surface of one of them be amalgamated, by spreading mercury over it with a piece of clean rag, so that it may become quite brilliant.* Both pieces being furnished with connecting wires, and in proper communication with the galvanometer, let them be plunged into a weak solution of either sulphuric or muriatic acid. The amalgamated piece will operate as zinc, and of course the other piece as copper in the standard battery. But it will be observed particularly, if the combination be placed in a glass vessel, that the piece which operates as copper undergoes *rapid destruction*, whilst the other is scarcely affected by chemical action. Gas will copiously ascend from the former, whilst a few indolent bubbles only, will be observed on the latter, which cling to its surface without making their escape. Hence it appears from this experiment, that the most oxidizable metal in a galvanic combination, does not universally operate as zinc in the standard battery or in other words it is not always the *positive metal.*†

* Zinc may be easily amalgamated by first dipping it in a solution of sulphuric acid, and afterwards in mercury.

† Sir H. Davy, in his Bakerian Lecture for 1826, observes that "zinc in amalgamation with mercury is *positive* with respect to pure zinc;" but he is perfectly silent as regards the nature of the chemical action which is developed by a combination of these materials in any acid solution; though, one might have supposed that, had he made the experiment, this striking and singular phenomenon could not have escaped the attention of so penetrating an observer, nor have been permitted to pass unnoticed whilst discussing the theories of galvanism.

Sir Humphry has also stated that, "There is not any inherent and specific property in each metal which gives it the electrical character; it depends

46. I know of no experiment that operates more decidedly against the chemical theory of galvanism than the one I have last described. The electricity displayed is uniform and steady from beginning to end; and its duration is determined by the durability of the *negative piece* (copper in the standard battery), and not by the other, as that theory supposes. I have observed a deflection of the needle of more than 10° for two successive hours, with two pieces, each exposing about one square inch of surface to the action of the acid solution; at the end of which time, the needle was perfectly steady at that angle, although the piece which operated as copper was nearly destroyed. On examining the amalgamated piece, very slight traces only of chemical action could be observed on its surface. The same amalgamated piece was successively combined with two others, which it likewise outlasted, still operating as zinc in the standard battery; and was even then much decayed by chemical action; but had become exceedingly brittle by combining with the fluid metal.*

47. *Iron and nitric acid.*—When diluted nitric acid is placed in one chamber, and water in the other, the piece of iron which is immersed in the acid solution, operates as copper, and consequently the other piece which is placed in the water, displays its electricity in the character of zinc in the standard battery.

When a few drops of acid are mixed with the water, the electrical energies become very much exalted; and the needle will frequently mark an angle of 35°, particularly if the stronger portion of

upon its peculiar state—on that form of aggregation which fits it for *chemical change.*"

From this statement of Sir Humphry's, I imagine that he has made no experiment like that described in the text; for it could never have been discovered from that experiment, nor with any other with which I am acquainted, that the amalgamation of zinc *exalts* its *oxidability* in a solution of either sulphuric or muriatic acid: the *most essential* "*chemical change*" required to satisfy the conditions of the electro-chemical theory.

* Were it not on account of the brittleness and other inconveniences occasioned by the incorporation of the mercury with the zinc, amalgamation of the surfaces of zinc plates in galvanic batteries would become an important improvement; for the metal would last much longer, and remain bright for a considerable time, even for several successive hours—essential considerations in the employment of this apparatus.

Notwithstanding the inconveniences, however, the improvement afforded by amalgamating the surfaces of zinc plates becomes available in many experiments; for the violent and intense chemical action which is exercised on zinc by a solution of sulphuric or muriatic acid, with the consequent evolution of heat, and annoying liberation of hydrogen, have no place when the plates are amalgamated: the action is tranquil and uniform, and the disengagement of gas, which is trifling, occurs only when the circuit is complete, and at the surface of the copper plate. The electric powers are highly exalted, and continue in play much longer than with pure zinc: and the only care of the experimenter is, to prevent the copper, or whatever metal be substituted, from becoming amalgamated.

With a solution of nitrous acid, the electrical energies of two pieces of zinc, the one pure, and the other amalgamated, are displayed in a very superior degree; but in consequence of the amalgamated surface becoming partially

the acid solution be not very feeble; and these energies seem to improve with an increase of acid in that portion of the fluid.

These electrical relations of the two pieces appear to be constant with every power of the acid solution, even from the first immersion, which is a peculiarity in this experiment that I have not observed, either with copper or zinc, for when those metals are separately employed with nitric acid, of different degrees of strength, the piece which is immersed in the stronger solution first displays its electricity in the character of zinc in the standard battery, and afterwards changes to that of copper, which is not the case with two pieces of iron, for they uniformly display the same electrical relations from the beginning to the end of the experiment.

48. When one piece of iron has been exposed for a short time to the action of a feeble solution of nitric acid, it will operate as zinc to another bright piece which is plunged in afterwards; the latter operating in the character of copper in the standard battery; but this species of action with iron is of very short duration, and the needle almost immediately returns to that direction which marks the last piece immersed to be operating as zinc in the standard battery.

49. *Iron and nitrous acid.*—The electrical relations of two pieces of polished iron when placed in two portions of this acid, very diffe-

oxidized, and liberating gas at its surface, the experiment is not so decisively opposed to the chemical theory of galvanism as when either muriatic or sulphuric acid is employed.

Two pieces of rolled zinc, each presenting ten square inches of surface, one of which was amalgamated, and made quite brilliant, were formed into a galvanic combination with nitrous acid diluted with twelve times its quantity of water, and connected with the galvanometer; the needle after several oscillations reposed at an angle of 65 degrees, after which the following results were observed, without in the least disturbing the apparatus:—

Minutes.		Degrees.
In	5 after the first immersion	70
—	20	68
—	30	65
—	45	60
—	60	55
—	75	50
—	90	45
—	105	40
—	120	35
In four hours		25

The metals were not disturbed for fifteen hours afterwards, at the end of which time the needle marked an angle of 18 degrees. An interruption was now made in the circuit without disturbing the metallic plates; when the needle had reposed in the magnetic meridian the circuit was again completed, and the needle deflected to an angle of 30 degrees, and became steady at 19 degrees.

The electrical powers displayed by two pieces of zinc, the one pure, and the other amalgamated, and a solution of nitrous acid, are sufficiently energetic to produce electro-magnetic rotations, even on a pretty large scale.

rently diluted, or the one piece in the acid solution, and the other in water, are precisely of the same character as when the nitric is employed, but the electrical energies displayed are more energetic. I have experimented with the acid and water in a variety of proportions, and the results are uniformly of the same character; the piece which is placed in the acid solution operates as copper, whilst that in the water, whether acidulated or not, displays the electrical character of zinc in the standard battery.

50. When both pieces are placed in the same acid solution, one a minute or two before the other, the latter, if bright, operates as copper in the standard battery very powerfully indeed; and this singular phenomenon is exhibited to as great an advantage with these materials as with any that I have noticed: but these electrical relations very soon cease, and the pieces almost immediately display electricity in the opposite way, precisely the same as when nitric acid is employed.

51. There is another phenomenon exhibited in these experiments which I believe has never before been noticed, but which, by the regularity of its display, must necessarily involve some theoretical principle, and consequently becomes as interesting to the philosopher as any other; I have observed it more or less in several other experiments, but as it is very decidedly exhibited with these materials I will describe it in this place.

52. When two pieces of polished iron have been for a few minutes immersed in a weak solution of nitrous acid, and in connexion with the galvanometer; if one piece be taken out, and very soon returned to its place, the needle will be deflected to a considerable angle, amounting in some instances to 90°, indicating the piece last plunged in to be operating as copper in the standard battery, and the needle will not return so quickly as if that piece had been bright before immersion, but will frequently retain that character for some time; it takes place with either piece, it is a matter of no consequence which is first plunged into the acid solution; the last will always display the phenomenon I have mentioned. I have obtained the same result for twenty successive times, by first taking out one piece, then the other, leaving them in the solution about half a minute between each time.

53. *Iron and muriatic acid.*—When two equal pieces of iron are immersed in a solution of muriatic acid, the piece last plunged in will display its electricity in the character of copper in the standard battery. If now, the other piece be taken out, it will also operate in the same capacity, in precisely the same manner as when iron and nitrous acid are employed; and this species of action takes place when the pieces are immersed, the one in the acid solution, and the other in water, so that the first effect indicated by the needle, will depend on the order of immersion; but if they be left unmolested for a minute or two, the piece in the acid solution will operate as copper, whilst that which is surrounded by water will operate as zinc in the standard battery;

and these electrical relations of the two pieces will be uniformly displayed while undisturbed in their respective chambers, but if either piece be in the *least moved* in the fluid, that piece will immediately operate as copper in the standard battery.

54. This singular and curious phenomenon, which I believe has not before been noticed, I shall endeavour to describe with some degree of minuteness; and likewise the process by which it appears to be the most decidedly exhibited.

Let two flat pieces of good iron, having each about two square inches of surface, exactly alike, and well polished, be connected with the galvanometer, and placed in a vessel containing muriatic acid diluted with two or three times its quantity of water. The needle will vibrate a little, but will soon come to rest; but, as it is next to impossible to select two pieces of iron so nearly alike in their electrical characters as not to display some galvanic effect, it is likely that the needle will not repose in the magnetic meridian, but will make some small angle therewith. Let that piece only, which the needle indicates to be operating as zinc in the standard battery be gently moved in the interposed fluid; the needle will immediately be deflected the contrary way, showing that the electrical relations of the two pieces of metal become changed by this process. When the needle has again come to rest, move the other piece, letting the first moved piece remain unmolested; the needle will again change its direction, and will indicate the last moved piece to be operating as copper in the standard battery.

55. If, whilst the connexions are complete, one of the pieces be moved rapidly to and fro in the acid solution, whilst the other remains at rest, the needle will be deflected to an angle of 40° or 50°, and may be kept steady at about 20° by continuing the motion, still indicating the moving piece to be operating as copper in the standard battery. But the moment the motion has ceased, the needle returns to the meridian, and very frequently takes a position on the other side.

56. I have tried iron with solutions of other acids, but cannot discover that decided effect as with the muriatic. I have also tried if the same phenomenon could be exhibited by employing other metals, such as copper, zinc, brass, &c., in different acid solutions, but I have failed to obtain any thing like that precision of results as are afforded by iron and diluted muriatic acid. In some cases indeed, the same process appears to operate in the contrary way; and particularly with tin in a solution of nitro-muriatic acid, as will be more particularly noticed in the sequel.

57. *Iron and sulphuric acid.*—I shall have very little to advance under this head, as the phenomena displayed are precisely of the same character as when muriatic acid is employed. There is not however that decided effect produced by agitating one of the pieces, as in a solution of muritic acid, but the result by that process is of the same nature.

From a retrospection of this complicated experiment, it will be observed that the results which I have obtained are very different from those stated in Mr. Ritchie's memoir. Perhaps some of those differences may have emanated from a dissimilitude in the mode of experimenting, or from that gentleman not having noticed those peculiar phenomena which I have minutely described; but there are certainly some discrepancies which I am persuaded no allowance of that nature can possibly reconcile.*

[We believe that the same pamphlet has some claims to priority of making known a few other facts, quite as interesting in these matters as those before alluded to. For instance, the non-necessity of metallic contact in voltaic pairs, and the superiority of *rolled* zinc over *cast* zinc, were first made known by that pamphlet.]

I do not, however, suppose with Volta, that the electricity developed by simple contact of the metals, exercises the extent of influence in galvanic arrangements which that philosopher imagined. I have interposed in various ways, pairs of copper and zinc, of two square feet in surface, in the galvanic circle, without observing the least effect in modifying the energies displayed by a *pair of wires* of the same metals, which were placed in an acid solution. And I have proved by experiments, which will be described in the sequel, *that electro-magnetic powers may be displayed without any metallic contact whatever.*

[So much for non-metallic contact. Now let us see what the pamphlet says about rolled and cast zinc:—]

68. Two pieces of zinc, each exposing two square inches of surface, the one *cast* and quite *hard* and *brittle;* the other *rolled,* but sufficiently *soft* and *pliable* to wrap round the figure like a strip of sheet lead, were placed in a glass vessel, and connected with the

* There is an advantage in separating the two portions of fluid by a bladder partition, which is not afforded by placing the two pieces of metal in separate vessels connected by moistened asbestos, as was the practice with Sir H. Davy; for it is a well known fact, that the *nearer* the metals approximate each other in the interposed fluid, the *greater* is the galvanic effect; and when the electrical energies are very feeble, it is necessary to give every facility to the display of the phenomena; hence it will be found, that when both pieces *press gently* against the opposite sides of the bladder, the needle will be deflected to a greater angle than when they are placed at a greater distance from each other. If unacidulated water be employed in one of the chambers, this precaution will be necessary to be attended to.

This circumstance alone, appears to me highly favourable to the opinion that, the energies of a galvanic combination are *exalted* in some proportion as the *conducting power* of the fluid medium becomes *improved;* for, by shortening the distance between the two plates in the acid solution, a portion of impeding obstacles to the transmission of electricity becomes removed, and the electric stream flows more copiously and with greater celerity by the facility thus afforded, which in fact, amounts precisely to the same thing, as if the distance were *constant*, and the *conductability* of the acid solution *improved.*

galvanometer by proper conducting wires; sulphuric acid was poured in, and a small deviation of the needle was observed, indicating the rolled piece to be operating as zinc in the standard battery. Water, to the amount of about three times the quantity of acid was now gently poured in; the chemical action became excessive, and the needle was deflected 30° in the same direction as at first. The following were the results of five minutes, which was the whole time that the pieces remained in the acid solution:—

	Degrees.	
First pouring in of the water	30	The rolling piece constantly operating as zinc in the standard battery.
In 1 Minute	38	
— 2 Minutes............	38	
— 3	37	
— 4	33	
— 5	30	

69. The metals being now brushed in clean water, were again introduced into the same acid solution. The same electrical relations of the two pieces were again displayed for five minutes, the angle of deflection being steady at 12°.

70. The same pieces were again brushed in clean water, and the experiment repeated with a fresh portion of sulphuric acid, diluted with three times its quantity of water; the metals were connected with the galvanometer, and the results again observed for five minutes, which were as follow:—

	Degrees.	
First plunge	180	The rolled piece uniformly displaying its electricity in the capacity of zinc in the standard battery.*
In 1 Minute steady at	45	
— 2 Minutes	47	
— 3	40	
— 4	35	
— 5	32	

* In experiments with these materials the chemical action is excessively intense for the first three minutes, about which time it generally subsides very rapidly.

The electrical energies appear to diminish from three causes. 1st.—As the chemical action subsides the height of the fluid medium subsides also, and therefore a less portion of metallic surface is exposed in the latter than in the former part of the experiment. 2nd.—The longer the pieces of zinc are exposed to the action of the acid, the more equal they become in the asperous character of their surfaces; consequently, their electrical energies are the most vigourous when the rolled piece is quite new and smooth, or well hammered to level the asperities on the surface—which experience demonstrates. 3rd.—Since by the first part of the experiment, *decomposition* is rapidly produced, a portion of the constituent parts of the fluid medium becomes either *determined* in the circuit, or entirely *expelled:* the latter part therefore, proceeds under very different circumstances, both as regards *quality*, *quantity*, and *arrangement* of the elements employed. This latter cause is common to all galvanic arrangements.

71. Experiments were instituted for the purpose of examining the electrical relations of two pieces of the same kind of *cast* zinc, the one hammered to level the asperities on its surface, but afterwards softened by annealing. The slips were parts of the same plate which was cast for the purpose, and prepared as above stated. When properly connected, and introduced into sulphuric acid diluted as before, the deflection of the needle amounted to 30°, but gradually decreased as the surface of the hammered piece became rough by corrosion, but continued for five minutes in the direction indicating the *soft hammered* piece to be operating in the character of zinc in the standard battery.

72. When the pieces of zinc were again cleaned with a brush and water, the hard piece was made rough by a rasp, both were again placed in the diluted acid used in the last experiment. The needle was first deflected in the direction indicating the rasped piece to be operating as zinc in the standard battery, which was a mere consequence of its superior brightness, and partial leveling of the natural asperities by the rasp; for the needle was immediately deflected the other way, and after a few oscillations it stood firmly at an angle of 15°, indicating the *soft smooth* piece to be operating as zinc in the standard battery, or in the same electrical character as it had done in the former experiment with these two pieces.

73. The experiment was varied by hammering the same piece again, in order to flatten the asperities which the action of the acid had produced on its surface, and afterwards annealing it to render it *soft* and *pliable*, the other piece still remaining *brittle* and *rough*.

When placed in their proper situations in sulphuric acid, diluted with three times its quantity of water, the electricity displayed was as powerful as at first, and precisely of the same character.

74. When the acid is diluted with thirty or forty times its quantity of water, the electricity displayed is much feebler than with stronger acid, but the metals uniformly exhibit the same electrical character, and the needle remains steady for a considerable time.

75. No one who becomes acquainted with these decisive results, can imagine that the *temper* of the metals had any part in varying their electrical relations. Some other cause must evidently exist, and which, I am persuaded, will be found in the irregularity of their surfaces; for it appears that in whatever way the experiments are varied, the piece which presents the smoothest surface to the diluted acid, displays its electric powers in the capacity of zinc; whilst the other piece which presents an infinitude of asperities, either from its natural texture, or by artificial preparation, uniformly becomes of the same electrical character as copper in the standard battery.

76. In a practical point of view, we readily become acquainted by these experiments, that there must be a considerable difference in the power of two galvanic batteries of equal size, when the zinc employed in their construction is in different states; for it is very

evident that as rolled zinc operates very powerfully in the capacity of zinc, when combined with another piece which is cast, but neither rolled nor hammered, plates of *rolled zinc* will therefore have an advantage over *cast plates* when combined with copper in the usual way. Hence, when the galvanic arrangement becomes very extensive, amounting to several hundred pairs, the additional power obtained by employing *rolled zinc* must be very great indeed.—Ed. Annals.

NOTICE.

It having been suggested by several readers of the "Annals," and also by some valuable private friends, that I should publish my RESEARCHES separately, as a distinct work, I am desirous of making known that those detached papers and memoirs already before the scientific world, in various periodicals, will be collected into one volume, which will be published early in the ensuing year. Such other discoveries as I have already made will form the *First Part* of a Second Volume, and will appear as early afterwards as convenience will admit: and should time and other circumstances allow of continuing my researches, the results will appear in a series of *Parts*, in continuation of the Second Volume.—W. S.

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